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Publication date:
2013

[Link to publication in Tilburg University Research Portal](#)

Citation for published version (APA):

Attanasio, O. P., Augsburg, B., de Haas, R., Fitzsimons, E., & Harmgart, H. (2013). *Group Lending or Individual Lending? Evidence from a Randomized Field Experiment in Rural Mongolia*. (CentER Discussion Paper; Vol. 2013-074). Finance.

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No. 2013-074

**GROUP LENDING OR INDIVIDUAL LENDING? EVIDENCE
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19 November, 2013

ISSN 0924-7815
ISSN 2213-9532

Group Lending or Individual Lending? Evidence from a Randomized Field Experiment in Rural Mongolia*

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Abstract

We present evidence from a randomized field experiment in rural Mongolia on the comparative poverty impact of group versus individual microcredit. We find a positive impact of group loans but not of individual loans on entrepreneurship and food consumption. Moreover, group borrowers are less likely to make informal transfers to families and friends while the opposite holds true for individual borrowers. This suggests that joint liability may deter borrowers from using loans for non-investment purposes with stronger impacts as a result. We find no difference in repayment rates between both types of microcredit.

Keywords: Microcredit; poverty; randomized field experiment

JEL Codes: O16, G21, D21, I32

*The authors thank Artyom Sidorenko, Veronika Zavacka, and Teodora Tsankova for excellent research assistance and Erik Berglöf, Robert Lensink, David McKenzie, Gabriel Natividad, David Roodman, Marta Serra Garcia, Jeromin Zettelmeyer, and participants at the Women for Women/J-Pal/EBRD Conference on ‘Banking on Women: Finance and Beyond’, the 2nd European Research Conference on Microfinance, the 2012 Royal Economic Society Annual Conference, the 7th New York Fed/NYU Stern Conference on Financial Intermediation, the 27th Congress of the European Economic Association, the ADB/IPA/J-PAL Impact and Policy Conference ‘Evidence on Governance, Financial Inclusion, and Entrepreneurship’, and seminars at the EBRD, National University of Mongolia, Frankfurt School of Finance & Management, J-PAL/Paris School of Economics, University of St. Gallen, University of Surrey, and XacBank for useful comments. This project would not have been possible without the tireless support from Ariunbileg Erdenebileg, Maria Lotito, Bold Magvan, Norov Sanjaajamts, and Benjamin Shell (XacBank); Oksana Pak (EBRD); Erin Burgess, Stephen Butler, Pamela Loose, and Jeffrey Telgarsky (NORC), and the Mongolian Women’s Federation.

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1 Introduction

The effectiveness of microcredit as a tool to combat poverty is much debated now that, after years of rapid growth, microfinance institutions (MFIs) in various countries are struggling with client over-indebtedness and repayment problems. This heightened skepticism also follows the publication of the findings of a number of randomized field experiments that indicate that the impact of microcredit might be more modest than advocated by its strongest proponents. These studies have tempered the expectations many had about the ability of microcredit to lift people out of poverty.

Yet, much remains unclear about whether, and how, microcredit can help the poor to improve their lives. Microcredit encompasses many different models and modalities and the evidence on the relative effectiveness and on the role played by different components is limited. Answering these questions is particularly important now that the microcredit industry is changing in various ways.

One important recent trend has seen increased scale and professionalization leading a number of established MFIs to move from group or joint-liability lending, as pioneered by the Bangladeshi Grameen bank in the 1970s, to individual micro-lending.¹ For this reason, establishing whether joint-liability loans or individual loans are more effective and/or sustainable is particularly salient.

Under joint liability, small groups of borrowers are responsible for the repayment of each other's loans. All group members are treated as being in default when at least one of them does not repay and all members are denied subsequent loans. Because co-borrowers act as guarantors they screen and monitor each other and in so doing reduce agency problems between the MFI and its borrowers. A potential downside to joint-liability lending is that it often involves frequent and time-consuming repayment meetings and exerts strong so-

¹Liability individualization is for instance at the core of 'Grameen Bank II'. Large MFIs such as ASA in Bangladesh and BancoSol in Bolivia have also moved towards individual lending. See also Cull, Demirgüç-Kunt, and Morduch (2009).

cial pressure, making it potentially onerous for borrowers. This is one of the main reasons why MFIs have started to move from joint to individual lending.

Somewhat surprisingly, there exists very limited empirical evidence on the relative merits of individual and group lending, especially in terms of impacts on borrowers. Both the ample theoretical and the more limited empirical literature mainly center on the impact of joint liability on repayment rates. Armendáriz and Morduch (2005, p. 101-102) note: *“In a perfect world, empirical researchers would be able to directly compare situations under group-lending contracts with comparable situations under traditional banking contracts. The best test would involve a single lender who employs a range of contracts (...). The best evidence would come from well-designed deliberate experiments in which loan contracts are varied but everything else is kept the same.”*

This paper provides such evidence from a randomized field experiment among 1,148 poor women in 40 villages across rural Mongolia. The aim of the experiment, in which villages were randomly assigned to obtain access to group loans, individual loans, or no loans from our implementing partner XacBank, is to measure and compare the impact of both types of microcredit on various poverty measures. Importantly, neither the group nor the individual-lending programs include mandatory public repayment meetings and are thus relatively flexible forms of microcredit.

The loans provided by the programs we investigate are relatively small, targeted at female borrowers, and progressive in nature: successful loan repayment gives access to another loan cycle, with reduced interest rates, as is the case with many microcredit programs. Our evaluation is based on two rounds of data collection: a baseline survey before the start of the lending programs and a follow-up survey 19 months later.

Although the loans provided under this experiment were intended to finance business creation, we find that in both the group- and individual-lending villages about half of all credit is used for household consumption rather than business

investment. Only among women that were offered group loans do we find an impact on business creation: the likelihood of owning an enterprise increases for these women by ten percent more than in control villages. In terms of poverty impact, we find a substantial positive effect of access to group loans on food consumption, particularly of milk, bread, and non-alcoholic beverages.

In terms of individual lending, we document no average increase in enterprise ownership. Amongst women in individual-lending villages we also detect no significant increase in (non-durable) consumption. The stronger and significantly different impact on consumption and business creation in group-lending villages may indicate that group loans are more effective at increasing the permanent income of households, though we detect no evidence of higher income in either individual- or group-lending villages, relative to controls.

If one were to take at face value the evidence on the larger impact of group loans, one would want to ask *why* such loans are more effective at raising consumption (and probably long-term income). One possibility is that the joint-liability scheme allows women to mutually insure each other against adverse business outcomes. This may reduce investment uncertainty, increase loan take up, and ultimately lead to larger long-run effects. We develop a concise theoretical model to further explore this hypothesis and provide supporting empirical evidence on the relationship between village risk and loan take-up. Our findings on informal transfers support this hypothesis too: women in group-lending villages decrease their transfers to families and friends, contrary to what we find for women in individual-lending villages. This suggests that risk sharing within lending groups may come at the cost of more informal risk-sharing arrangements with (other) family members and friends.

This paper provides a comparative analysis of individual versus joint-liability microcredit and as such is related to the theoretical literature on joint-liability lending that emerged over the last two decades.² Notwithstanding the richness

²See Ghatak and Guinnane (1999) for an early summary. Theory suggests that joint liability

of this literature, the impact of joint liability on risk taking and investment behavior remains ambiguous. For instance, on the one hand group lending may encourage moral hazard if clients shift to riskier projects when they expect to be bailed out by co-borrowers. On the other hand, joint liability may stimulate borrowers to reduce the risk undertaken by co-borrowers since they will get punished if a co-borrower defaults. Giné, Jakiela, Karlan, and Morduch (2010) find, based on laboratory-style experiments in a Peruvian market, that contrary to much of the theoretical literature, joint liability stimulates risk taking - at least when borrowers know the investment strategies of co-borrowers. When borrowers could self-select into groups there was a strong negative effect on risk taking due to assortative matching. Fischer (2013) undertakes similar laboratory-style experiments and also finds that under limited information, group liability stimulates risk taking as borrowers free-ride on the insurance provided by co-borrowers (see also Wydick (1999) for empirical evidence from Guatemala on intra-group insurance). When co-borrowers have to give upfront approval for each others' projects ex ante moral hazard is mitigated.

Giné and Karlan (2010) examine the impact of joint liability on repayment through two experiments in the Philippines.³ They find that removing group liability, or introducing individual liability from scratch, did not affect repayment rates over the ensuing three years. In a related study, Carpena, Vole, Shapiro, and Zia (2013) exploit a quasi-experiment in which an Indian MFI switched from individual to joint-liability, the reverse of the switch in Giné and Karlan (2010). They find that joint liability significantly improved repayment rates. Our paper is the first to use the same experimental context to compare the impact of individual versus joint-liability microcredit on borrowers.

may reduce adverse selection (Ghatak, 1999/2000 and Gangopadhyay, Ghatak, and Lensink, 2005); ex ante moral hazard by preventing excessively risky projects and shirking (Stiglitz, 1990; Banerjee, Besley, and Guinnane, 1994 and Laffont and Rey, 2003); and ex post moral hazard by preventing non-repayment in case of successful projects (Besley and Coate, 1995 and Bhole and Ogden, 2010).

³Ahlin and Townsend (2007) empirically test various repayment determinants in a joint-liability context in Thailand.

The paper proceeds as follows. In Section 2 we describe the loan program and target population after which Section 3 sets out our experimental design. Section 4 then discusses our main results while Section 5 provides some evidence about the relationship between village risk and loan take-up and presents a simple model that can be helpful in interpreting our results. Section 6 concludes.

2 The program

2.1 Target population

Microfinance as it is known today originated in Bangladesh - one of the most densely populated parts of the world with 1,127 people per km² - but has also taken hold in less-populated countries. One of these is Mongolia, which encompasses a land area half the size of India but with less than 1 percent of the number of inhabitants. This makes it the least densely populated country in the world with just 1.7 people per km².⁴ This low population density means that disbursing, monitoring, and collecting small loans to remote borrowers is costly, particularly in rural areas. Mongolian financial institutions are therefore constantly looking for cost-efficient ways to service such borrowers.

Mongolian microcredit has traditionally been provided as individual loans, reflecting concerns that the nomadic lifestyle of indigenous Mongolians had impeded the build-up of social capital outside of the family. Notwithstanding such concerns, collective self-help groups (*nukhurlul*) have developed and some of these have started to provide small loans to their members, in effect operating as informal savings and credit cooperatives. This indicates that group lending might be feasible in rural Mongolia. Moreover, recent theoretical work suggests

⁴Source: United Nations World Population Prospects (2005). Mongolia has a semi-arid continental climate and an economy dominated by pastoral livestock husbandry and mining. Extreme weather conditions - in particular droughts and harsh winters with temperatures falling below -35° C (-31° F) - frequently lead to large-scale livestock deaths.

that when group contracts are sufficiently flexible, group loans can be superior to individual loans even in the absence of social capital (Bhole and Ogden, 2010). Group lending may therefore also work in countries where social connectedness and the threat of social sanctions is relatively limited.

We conducted our experiment in cooperation with XacBank, the second largest provider of microcredit in Mongolia. While XacBank provides both men and women with microcredit, our experiment focused on extending credit to economically disadvantaged women in rural areas. With the exception of a few small NGOs, microcredit in Mongolia is provided by two commercial banks, Khan Bank and XacBank. Khan Bank has an extensive branch network which it inherited from the communist era and is present in most Mongolian villages. Its rural branches are very small and typically located in the same building as the local government administration. According to the Mongolian National Statistics Office (2009) these small branches have little incentive to diversify their lending and mainly lend to relatively well off herders with high-quality collateral in the form of large flocks of animals.

At the time of our experiment, poorer (and in particular female) borrowers were almost entirely excluded from access to business credit. According to the Mongolian National Statistics Office (2006, p. 54): “*Microcredit appears to be unavailable to most of the poor living in the aimag and soum centers.*⁵ *Their normal channels for credit are to borrow from a shop or kiosk (...) or from a relative or friend*”. Indeed, at the time of our experiment 78 per cent of all bank lending (including microcredit) took place in Mongolia’s capital Ulaanbataar even though only 36 per cent of all Mongolian households resided in that urban area (IFC-KfW, 2009). The only types of credit available to rural women were

⁵Mongolia is divided into 18 aimags or provinces which are subdivided into 342 soums or districts. Each soum contains a small village or soum center of on average one kilometer in diameter. The average soum in our experiment had 3,853 inhabitants of which 1,106 lived in the central village. The average distance from a village to the nearest province center - small towns where XacBank’s branches and loan officers are based - is 116 kilometers. Because the distance between a village and the nearest paved road is on average 170 km, travel between villages, and between villages and province centers, is time consuming and costly.

informal loans or small consumer loans -backed by pensions or salaries- that were typically used to buy mobile phones or small electrical appliances. The purpose of XacBank's expansion into a large number of villages was to open up access to business credit for this hitherto excluded group of poor rural women.⁶

2.2 The loan products

The purpose of both group and individual loans was to allow women to finance small-scale entrepreneurial activities.⁷ Given the focus on business creation and expansion, loans had a grace period of either two months (loans exceeding six months) or one month (shorter loans).⁸ The interest rate varied between 1.5 and 2 percent per month and was reduced by 0.1 percent after each successful loan cycle. Other dynamic incentives included the possibility to increase the loan amount and/or maturity after each repaid loan (Appendix Table A2).

Group-loan contracts stated that loans were based on joint liability and that XacBank would terminate lending to the whole group if a group did not fully repay a loan. Most group loans were composed of individually approved sub-loans with a maturity between 3 and 12 months depending on the loan cycle (within a group all sub-loans had the same maturity). Groups could also apply for a joint loan to finance a collective business, for instance to grow crops. The maximum size of the first loan to a group member was MNT 500,000 (\$435). Group members had to agree among themselves who would get a loan and for what purpose. They then had to apply for the loan and XacBank screened each application independently.⁹ If a borrower's project was deemed too risky XacBank could

⁶According to its mission statement, XacBank intends to maximize shareholder value and to foster Mongolia's socio-economic development by providing access to comprehensive financial services to citizens and firms, including those that are normally excluded, such as low-income and remote clients.

⁷Besides agriculture - both animal husbandry and crop growing - the main village industries are baking, wood-processing, retail activities, and felt making.

⁸Field, Pande, and Papp (2010) provide evidence from a randomized field experiment in India that the flexibility associated with a two-month grace period - instead of the regular two weeks - led to more business creation and investments but also to lower repayment rates.

⁹Loan officers were all female, between 21 and 27 years old, married with one or two chil-

exclude her while the other members would still get a loan. If most projects were judged to be too risky then the total group loan was rejected. The screening of group loans thus involved a two-stage process: first by co-borrowers and then by a XacBank loan officer.

Before applying for a loan, groups had to build up savings in a joint savings account equivalent to 20 percent of the requested loan amount. Group members were in principle allowed to pledge assets instead of the compulsory savings although XacBank encouraged borrowers to use savings. The savings not only served as collateral but were also a means of ascertaining whether potential borrowers had sufficient financial discipline.

Group leaders were responsible for monitoring and collecting repayments and handing them over to the loan officer each month. There were no public repayment meetings or other mandatory gatherings.¹⁰ Groups decided themselves on the modalities of their cooperation, including whether to meet regularly or not, and if so, how frequently (typically once per month). The group loan product was therefore more flexible than “traditional” group lending, which borrowers often consider burdensome due to the associated frequent and lengthy repayment meetings (e.g. Wydick, 2001).¹¹

Individual loans were similar to the sub-loans provided to group members, though larger on average.¹² XacBank did not use strict collateral requirements

dren, and had completed at least a four-year university degree. They normally assess between 35 (Hentii province) and 50 (Hovsgol province) loan applications per month with an approval rate of about 90 percent.

¹⁰Field and Pande (2008) randomly assign weekly or monthly repayment meetings (for individual-liability loans) and find that a lower-frequency schedule can significantly reduce transaction costs without increasing defaults. However, building on the same experiment, Feigenberg, Field, and Pande (2013) show that more frequent repayment meetings have a positive impact on borrowers’ social capital and their pro-social behavior. In the longer term this resulted in lower default rates on borrowers’ second loans (even though all borrowers had by that time reverted to the same repayment schedule).

¹¹Note that in Giné and Karlan (2010) weekly meetings were held in both individual-lending and group-lending villages.

¹²Theory suggests that lenders may aim to counterbalance the adverse incentive effect of joint liability (that is, moral hazard makes borrowers choose riskier projects) by offering smaller loans to joint as compared to individual-liability borrowers (Madajewicz, 2011).

but took collateral if available. As a result 91 percent of the individual loans were collateralized, with the average collateral value close to 90 percent of the loan amount. The maturity of individual loans ranged from 2 to 24 months, depending on the experience of the borrower and the type of business. Group loans had a somewhat shorter maturity (192 days on average) than individual loans (245 days) which reflects their smaller size. Similar to group loans, individual loans did not involve any mandatory group activities such as repayment meetings.

Appendix Table A2 also shows that women used the individual and group loans in very similar ways. Assuming that the purchase of livestock, tools, and machinery are business expenses, we find that 66 (67) percent of individual (group) borrowers used their first loan mainly to invest in a new or existing enterprise, putting between 70 and 80 percent of the loan to this purpose, with the remainder being used for household expenses. In the case of second loans, fewer women - 52 (43) percent of the individual (group) borrowers - used the loan primarily for business purposes.

We can also compare what women reported as the purpose of the loan at baseline and at follow-up. We find that 93 (86) percent of individual (group) borrowers who at follow-up stated that they had used their loan(s) mainly for business purposes, had consistently indicated at the start of the experiment that they would use the loan for entrepreneurial activities. However, 82 percent of women in both types of treatment villages who used the loan mainly for consumption had reported at baseline that they would use it to invest in a business. We cannot say whether they intentionally misreported at baseline (as the loans were marketed as business loans) or whether they later changed their minds due to unforeseen circumstances.

3 The experiment

3.1 Experimental design and roll-out of the lending programs

The experiment took place in 40 *soum* centers (henceforth: villages) across five aimags (henceforth: provinces) in northern Mongolia. Figure A1 in the Appendix maps the geographical location of all participating provinces and villages. The experiment started in February 2008 when XacBank loan officers and representatives of the Mongolian Women's Federation (MWF) organized information sessions in all 40 villages. The MWF is a large non-governmental organization (NGO) whose representatives worked together with the research team to ensure a smooth implementation of the experiment. They signed up participants, facilitated group formation in the group-lending villages, provided information to loan applicants, and assisted the survey company.

The goal and logistics of the experiment were explained and it was made clear to potential borrowers that there was a 75 percent probability that XacBank would start lending in their village during the experiment and that lending could take the form of either individual or group loans. In particular, out of the 40 study villages, 15 would be allocated to group lending, 15 to individual lending and 10 to control status. Women who wished to participate could sign up and were asked to form *potential* groups of about 7 to 15 persons each. Because of the explicit focus on relatively poor women, the eligibility criteria stated that participants should own less than 1 million Mongolian *tögrög* (MNT) (\$869)¹³ in assets and earn less than MNT 200,000 (\$174) in monthly profits from a business. Many of these women were on 'poor lists' compiled by district governments. The local MWF representatives were responsible for the enforcement of the eligibility criteria. We were able to check compliance by using the information on asset ownership and income that we collected during the baseline

¹³We use an exchange rate of 1,150 MNT/\$, the average rate during the first half of 2008.

survey. This indicated that, with only very few exceptions, the eligibility criteria had been enforced and that relatively poor women had been selected to get a chance to gain access to microcredit.

Indeed, various indicators show that the households in our sample lie markedly below the Mongolian average in terms of income, expenditures, and social status. Data from the Mongolian statistical office indicate that the average rural household in 2007 had an annual income of MNT 3,005,000 (\$2,610) whereas the average household in our sample only earned MNT 1,100,000 (\$955) (we define earnings as entrepreneurial profits plus wages from formal employment by all household members. Social benefits are excluded). Similar patterns emerge when we compare expenditures using data from the Mongolian statistical office or from the EBRD-World Bank 2006 Life in Transition Survey, or when we compare livestock ownership, a primary wealth indicator in Mongolia.

After about 30 women had signed up in each village and had declared their interest in receiving a loan, a detailed baseline survey was administered to these women during the second and third week of March 2008 (Figure A2 in the Appendix provides a detailed timeline of the experiment). There were three survey teams in the field simultaneously to ensure that respondents in the three types of villages were interviewed at the same point in time.¹⁴ The total number of women interviewed was 1,148. The face-to-face interviews were conducted by a specialized survey firm hired by the research team and independent of XacBank.

Interviews were held at a central location in each village where respondents and interviewers had sufficient time to go through the questions without interruptions. A central location also minimized the risk that the female respondents would give biased answers due to the presence of older and male family members (as had happened during piloting). Interviews lasted approximately one hour and the interviewing process took on average two days per village. At the

¹⁴Each team handled all three types of villages in their area. Statistical tests show no significant differences in the timing of the baseline interview across village types.

time of the baseline survey we also collected information on the main socioeconomic, demographic, and geographic characteristics of the 40 villages.

The baseline survey measured variables that reflect households' living standards and that could be expected to change over the 1.5 year interval of the experiment. These include income, consumption, and savings; entrepreneurial activity and labor supply; asset ownership and debt; and informal transfers. We also elicited information about household composition and education; exposure to economic shocks; and respondents' subjective income expectations. Finally, we collected information on more context-specific poverty indicators such as livestock ownership and the quality and size of the dwelling, most often a *ger*.¹⁵

Randomization took place *after* completion of the baseline survey so that at the time of the interview respondents did not know whether they would be offered a group loan, an individual loan, or no loan at all. Randomization was performed in a central location, using a random number generator in Stata, as decentralized and public randomization was not feasible given that the villages were spread over a large geographical territory. We randomized at the village level, with 15 villages receiving access to individual loans, 15 receiving access to group loans, while in 10 control villages XacBank did not provide loans to the participating women for the duration of the experiment. In all three types of villages XacBank continued to provide individual micro-loans to regular, more wealthy clients most of whom were male. Randomization across rather than within villages was chosen because it was administratively and politically easier to manage. Moreover, randomization across villages avoids the possibility that the program affects, through informal transfers and connections, even individuals who do not receive credit directly. We stratified at the province level because a completely randomized design could have resulted in a situation

¹⁵A *ger* is a portable tent made from a wood frame and felt coverings. Its size is measured by the number of lattice wall sections (*khana*) and a basic *ger* consists of four or five *khana*. Bigger *gers* are a sign of wealth as they are more costly to heat. A sufficiently insulated *ger* has two layers of protective felt whereas poorer households often only have one layer. *Gers* are sometimes surrounded by (costly) wooden fences (*hashaa*) that offer protection from the wind.

whereby some provinces contained only treatment or control villages, which was unacceptable to XacBank. Also, to the extent that geographical or economic differences between provinces are large, we might not have been able to detect treatment differences in an unstratified design.

After randomization, group formation proceeded in the 15 group-lending villages, but not in the individual-lending and control villages. Group formation consisted of the development of internal procedures, the election of a group leader, and the signing of a group charter. Groups were formed by the women themselves not by XacBank. A maximum of two women per group were allowed to be from the same family. Group members lived in the same village and already knew each other to varying degrees. In many cases actual group composition differed substantially from the potential groups that were identified at the very beginning of the experiment when women had to indicate their interest to participate in the project. After a group had collected enough internal savings it could apply for its first XacBank loan.

The treatment period during which XacBank provided loans in the group and individual-lending villages lasted 1.5 years - from March 2008 to September 2009. During this period participating women in treatment villages could apply for (repeat) loans¹⁶, while XacBank refrained from lending in the control villages. Because XacBank started marketing its loans in both types of treatment villages at the same time, there is no significant difference between village types in the time lag between the start of the baseline survey and the disbursement of the first loan (p-value: 0.90).

In October 2009 we conducted a follow-up survey to measure again the poverty status and economic activity of the participating women. We also obtained information on how women had used their XacBank loan(s). There were again no significant timing differences in the interview process: the amount of time between the day that the first loan was distributed in a village and the day

¹⁶Of all borrowers 47 per cent received at least one repeat loan during the experiment.

that the follow-up survey started does not differ significantly between group-lending and individual-lending villages (p-value: 0.89) and was on average 13.7 months. Compared to the baseline survey, the interview process now took longer (on average 20 days per village) as interview teams had to spend more time on tracking respondents or waiting for them to return to the village (the time spent on the interviews themselves was practically the same as during the baseline survey). The follow up survey also included new questions eliciting the distribution of subjective expectations about returns to investment activities, which we describe below.

At this time, we also conducted a second village-level survey to collect information on village characteristics that may have changed, such as the prices of important consumer goods. XacBank collected repayment information on all loans for the entire period March 2008-June 2011. Lastly, in October 2011, we revisited one individual-lending and two group-lending villages for structured interviews and discussions with a number of borrowers about their experience of the lending programs.

3.2 Treatment-control balance

Table 1 presents a statistical comparison between the control villages and the two types of treatment villages. For each variable we present the baseline mean for the control group (in the post-attrition sample) as well as the difference in means between the control and both treatment groups (with a p-value for a t-test of equality of these means).

This shows that the respondents in the control and both types of treatment villages were very similar overall in terms of household composition, employment and consumption patterns (Panel A). Households are also very similar in terms of a large number of other consumption and asset-ownership measures (available upon request). As we consider many variables, we expect some statistical differences between the groups and this is the case for access to fi-

nance at the household level. A majority of the households had at least one loan at the time of the baseline survey and this percentage is higher in the individual-lending villages (69 percent) than in the control villages (57 percent) (p-value: 0.07). However, Table 1 also shows that in terms of the average amount borrowed (unconditional on borrowing) these differences between both village types are insignificant. A minor exception is that in individual lending villages the average amount borrowed from sources other than banks or informal lenders - in the Mongolian context this mainly leaves savings and credit cooperatives - is slightly higher (MNT 3,850 or about USD 3; p-value: 0.10). For the group villages we see that while the probability at baseline of having a loan from a bank was similar as in the control villages, the average loan size was somewhat higher (p-value: 0.07).

These figures also indicate that at the time of our baseline survey the penetration of small retail-type loans was relatively advanced in rural Mongolia.¹⁷ For our purposes, an important question is whether households were also using this credit to finance entrepreneurial activities by our female respondents. Our baseline data show that this appears not to be the case. Around 75 percent of all outstanding loans were used for consumption, mainly to buy electric household appliances, instead of income generation. This picture is the same across all types of villages at baseline. Second, fewer than 20 percent of households had invested part of their loan(s) in a business owned by the female targeted by the loan. Furthermore, the amount and percentage of funds used for female enterprises did not differ significantly between the three types of villages. In control villages households had invested on average 19 percent of their outstanding debt in a female-run business, whereas these percentages were 15 and 14 percent in individual and group-lending villages. These percentages, as well as the absolute amounts, do not differ significantly between control and treatment villages.

¹⁷Most poorer households and women in rural areas only had access to short-term consumer credit, backed by non-entrepreneurial income sources such as pensions or salaries, that was used to buy small household goods.

[Insert Table 1 here]

We conclude that the randomization process appears successful: we find very few significant differences between treatment and control villages, despite considering a broad range of variables. The few differences that do exist are small and do not provide evidence of a systematic disparity between treatment and control villages along any particular dimension. We are therefore confident that randomization ensured absence of selection bias so that we can attribute any post-treatment differences in outcomes to the lending programs.

3.3 Attrition

The follow-up survey took place approximately 1.5 years after the baseline survey and 84 percent of respondents were successfully re-interviewed. A possible concern is that non-response was not random across treatment and control villages, which could bias the estimated treatment effects. Table 1 shows that respondents in individual-lending villages were 7 percentage points more likely to attrit compared with those in control villages (p-value: 0.08). To investigate this in more detail, we estimate the probability of attrition as a function of treatment dummies as well as a range of respondent, village, and household characteristics. These (unreported) regressions show that these characteristics have the expected association with attrition: respondents that own a fence or a well and families with more women and small children are less likely to attrit - as one would expect, given that these characteristics are generally associated with less mobility. Households that live further from the province center and/or own horses or camels are more likely to attrit, presumably because they are more likely to live a semi-nomadic lifestyle and are thus more difficult to locate for interviews. Households that experienced a recent death were less likely to participate in the follow-up survey too.

A closer inspection of the data shows that the differential attrition is driven by two individual-lending villages where the wedding season was underway at the time of the follow-up survey, resulting in many respondents being away from home temporarily. We are thus reassured that the reason for higher attrition is unlikely to be related to the program, and so we retain these two villages in the analysis. While one might think that loan use might be distorted due to the wedding season, we also estimate all models excluding these two villages and find that our results are robust.

3.4 Estimation approach

In what follows, we report the results of an intention to treat (ITT) analysis where we compare *all* women who initially signed up in treatment villages, irrespective of whether they borrowed or not, with those who signed up in control villages.¹⁸ The advantage of this conservative approach is that we can interpret the experimental intervention as a policy and learn about the impact on the population that XacBank initially targeted, and not just on those who actually borrowed. We also employed an instrumental variables (IV) methodology in which we instrument *actual* borrowing status of participants with a dummy indicating whether or not the village was randomized to be a treatment village. These IV results are very similar to the ITT findings described below and are available on request.

The results reported here are based on a difference-in-differences technique in which we compare all (reinterviewed) respondents in treatment and control villages before and after the loan treatment.¹⁹ Given that the sample is balanced,

¹⁸One can calculate the impact of access to microcredit on those women who actually borrowed - i.e. the average effect of the treatment on the treated (ATT) - by dividing the ITT effect by the probability of receiving treatment (57 percent in the group-lending villages and 50 percent in the individual-lending villages). A caveat is that this may not generalize as those who receive the treatment may be systematically different from those who do not. As the assumption underlying consistent estimation of ATT is that unobservable characteristics do not affect the decision to participate, we only show ITT parameters.

¹⁹We use OLS (a probit model) for continuous (binary) dependent variables.

the only reason to use difference-in-differences is to increase the precision of the estimates by taking baseline characteristics into account that are strong determinants of the outcome variables.²⁰ As our sample was balanced at baseline, the question whether one should use post-treatment data only or a difference-in-difference approach boils down to whether the variance of time-invariant individual effects is greater or smaller than the variance of transitory shocks.²¹ If the former is smaller, using post-treatment data only would be the appropriate strategy. If it is greater, we should use difference-in-differences. Ex-ante we do not have information on the relative size of these variances. Most of our results are not affected by the choice of simple or double differences. As the precision of our estimates increases slightly when we use the diff-in-diff approach we report these results.²²

Statistical power can be held back in case of high intra-cluster correlation and only a limited number of clusters (in our case villages). Throughout the paper we therefore report cluster-robust standard errors. Since we have 40 clusters (that is, more than the often used rule of thumb of 30) cluster-robust standard errors should in principle be sufficiently conservative (see also Bertrand, Duflo, and Mullainathan, 2004). We nevertheless also calculate standard errors based on the even more conservative wild cluster bootstrap-t procedure (Cameron, Gelbach, and Miller, 2008). All our main results continue to hold at the same significance levels if we use these bootstrapped standard errors.

Our basic regression framework is:

²⁰We do not include strata dummies (aimag fixed effects) as additional covariates since fixed aimag level characteristics are differenced out in the diff-in-diff setting. When we include strata dummies, the results are quantitatively and qualitatively very similar as standard errors remain virtually unaffected. See also Bruhn and McKenzie (2009).

²¹Difference in-differences may limit statistical power if autocorrelation in the outcomes is limited (McKenzie, 2012). Note that in our case autocorrelation varied considerably across outcome variables (enterprise ownership: 0.35, hours worked: 0.41, total consumption: 0.30, herd size: 0.76, ownership of large household appliances: 0.50).

²²We also estimated impacts using post-treatment data only and an ANCOVA analysis which controls for the baseline value of the outcome. Results are similar, though less precisely estimated, and available upon request. When discussing our findings we mention when results differ with the alternative method.

$$Y_{ivt} = \alpha_0 + I_v \cdot (\alpha_1 + \alpha_2 \cdot F_t) + G_v \cdot (\alpha_3 + \alpha_4 \cdot F_t) + \alpha_5 \cdot F_t + \alpha_6 \cdot X_{i0} + \varepsilon_{it} \quad (1)$$

where:

- Y_{ivt} is the outcome variable of interest for individual i in village v at time t ($t = 0$ (1) at baseline (follow-up) survey);
- I_v is a binary variable equal to 1 for individual-lending villages (0 otherwise);
- G_v is a binary variable equal to 1 for group-lending villages (0 otherwise);
- F_t is a follow-up binary variable (0 for baseline observations);
- X_{i0} is a set of baseline characteristics of respondents and their households;
- ε_{it} is an i.i.d. error term clustered at the village level.

In this specification α_2 and α_4 measure the impact of the individual and group lending treatment, respectively. In addition, we also run more flexible specifications where we allow for heterogeneous impacts by education level of the respondent, which we consider to be an indicator of long-term poverty of the household. Since poverty reduction was the main goal of the microcredit programs, analyzing impact heterogeneity across education levels is a logical step. We estimate:

$$\begin{aligned} Y_{ivt} = & \alpha_0 + I_v \cdot (\alpha_1 + \alpha_2 \cdot F_t \cdot L_i + \alpha_3 \cdot F_t \cdot H_i) \\ & + G_v \cdot (\alpha_4 + \alpha_5 \cdot F_t \cdot L_i + \alpha_6 \cdot F_t \cdot H_i) \\ & + \alpha_7 \cdot F_t + \alpha_8 \cdot F_t \cdot L_i + \alpha_9 \cdot F_t \cdot H_i + \alpha_{10} \cdot X_{i0} + \varepsilon_{it} \end{aligned} \quad (2)$$

where H_i is one for individuals with a high education level (grade 8 or higher, or vocational training) and zero for individuals with a low education level (less than

grade 8) and L_i is one for individuals with low education and zero otherwise. All other variables are as previously defined. The coefficients of interest in this specification are α_2 and α_3 , which measure the impact of individual lending for low and high educated women, respectively, and α_5 and α_6 which measure the impacts for low and high educated women in group villages. As shown in Table 1, at baseline education levels were well balanced between the control villages and the two types of treatment villages.

4 Results

In this section we report our main results. We start with loan take ups, to move on to self-employment and income, hours of work and consumption and saving. We conclude the section with results on schooling and informal transfers.

4.1 The intervention and access to liquidity

After the baseline survey XacBank started disbursing individual and group loans in the respective treatment villages. All women who had signed up and expressed an initial interest in borrowing were visited by a loan officer and received a first loan after a successful screening. Although other banks (primarily Khan Bank) were also lending in both the treatment and control villages during the experiment, our intervention led to a significant increase in borrowing. Column 4 in Table 2 shows that the probability of receiving any type of loan during the experiment was substantially higher in treatment than in control villages: 71 (77) percent in individual (group) lending villages versus 50 percent in the control villages.²³ Column 1 of Table 2 confirms that the large difference in loan take-up between the treatment and control villages is driven by XacBank's lend-

²³By way of comparison, Banerjee et al. (2013) report a difference in short-term take-up between treatment and control group of 8.3 percentage points while in the study by Crépon et al. (2011) the difference was 10 or 16 percentage points depending on whether MFI or survey data are used.

ing programs. While even in the control villages a small number (6 percent) of respondents reported to have received a XacBank loan, these numbers are much higher for the individual and group lending villages, at 50 and 57 percent, respectively. In both types of treatment villages loan take up was very similar among low versus high-educated women. The strategy of inviting people to sign up and express an initial interest in microcredit therefore led to a relatively large difference in take-up between treatment and control villages, resulting in additional statistical power. Of course, this strategy also defines our population of interest more narrowly as those that were interested in accessing microcredit in the first place.

Notwithstanding the relatively large difference in borrowing activity between treatment and control villages, we note that after 1.5 years on average only 54 percent of all treatment respondents -all of whom had indicated an interest in microcredit during our initial information sessions- had borrowed from XacBank. This percentage was lower in individual lending villages (50 per cent) than in group-lending villages (57 percent). Moreover, not only the probability of borrowing but also the intensity of the treatment in terms of number of loans provided was higher in group villages. The mean number of loans was 0.99 in group-lending villages and 0.57 in individual-lending villages. This means that conditional on borrowing women took out on average two (consecutive) loans in the group-lending villages and one in the individual-lending villages. We already discussed that there were no economically or statistically meaningful differences between both village types in terms of the timing of the interviews or the *start* of the roll-out of XacBank's lending programs. However, on average loans were disbursed 46 days later in the individual villages compared to the group villages (p-value: 0.06). This suggests that the (s)lower loan take-up in individual-lending villages is mainly the result of lower demand. Indeed, this is confirmed when we use information collected through the follow-up survey to better understand why a significant proportion of women in treatment villages

did not borrow. These data show that about 75 percent of the ‘non-treatment’ was due to women who either did not apply for a loan or who applied but subsequently refused the offer. This leaves only about a quarter of all ‘untreated’ women who were actually refused a loan by XacBank.

The last four columns of Table 2 provide some insight into late repayment and default behavior. Columns 5 and 7 are based on administrative data from XacBank while columns 6 and 8 are based on respondents’ answers to our follow-up survey. Columns 5 and 6 show that defaults on XacBank loans were minimal in both types of treatment villages. Delayed repayment (here defined as loans that were at any time at least 90 days late) occurred in about 5 (7) percent of all individual (group) loans (this difference is not statistically significant). In both types of treatment villages late repayment is concentrated among the higher-educated borrowers. As we will show later, most enterprise creation was concentrated among the lower-educated women (in group villages) whereas higher-educated women used more of the loans to buy household goods (in individual villages). We analyze the relationship between village risk, loan take up, and loan repayment in more detail in Section 5.

[Insert Table 2 here]

4.2 Impact on self-employment and income

A key objective of the microcredit programs was to encourage women to expand or invest in small-scale enterprises, with the ultimate aim of reducing poverty and improving well-being. To evaluate the extent to which the programmes achieved these two objectives, we first look at the effect on enterprise creation and growth, and on whether enterprise profits and income more generally increased. We then go on to estimate the effect on detailed household consumption, as a measure of well-being. To pre-empt, we find evidence of households

in group villages increasing investment in enterprises, and corresponding increases in consumption. We detect no systematic impacts in individual villages on these outcomes.

In Table 3, and all other impact tables, we first show treatment effects in the individual-lending villages (upper panel) and then in the group-lending villages (lower panel). In each case we first present overall impacts and then impacts for women with low education (*Low*) versus those with higher (*High*) education. All of the underlying difference-in-differences regressions include a set of baseline respondent characteristics (listed in Table A1 in the Appendix). Throughout the remainder of this paper, whenever we report divergent impacts for respondents in group versus individual villages, these differences are statistically significant at the 10 percent level or lower unless stated otherwise.

Column 3 in Table 3 shows the impact of access to microcredit on the probability that the household operates a small-scale business, which could be owned by the respondent, her spouse, or by the couple jointly (65 percent of respondents are married or cohabitating). Among lower-educated group borrowers access to credit led to an 18 percentage points higher probability of entrepreneurship, at the household level, compared to the control group. This is also reflected in increased business asset ownership as measured by an index that captures whether the household owns tools and machinery; a tractor or lorry; riding equipment; and/or unsold stock (column 2). We find no significant impact on the (self-reported) total value of these assets (column 1).

[Insert Table 3 here]

Column 6 then shows that access to group loans had a positive impact on female entrepreneurship in particular. We find an overall increase of 10 percentage points (compared to an end-line level of female entrepreneurship of 39 percent in the control group). This effect is driven by less-educated women: at the

end of the experiment these women had a 31 percentage points higher chance of operating a business compared with women in control villages.²⁴ We do not find that access to credit resulted in more profitable enterprises or changes in other sources of household income (Table 4).

[Insert Table 4 here]

4.3 Impact on hours worked

In Table 5 we look at whether households increased their labor supply as a result of the lending programs. While we find no impacts in the individual-lending villages, we do find an increase in weekly hours worked on the female-run business in the group-lending villages (column 2). In line with our previous results, this effect is concentrated among the lower-educated women. Within these households, adults spend an average 19 more hours on the female-run business compared to the control villages (column 10). This implies a doubling of the number of hours worked on the female business compared to the control group. Column 7 also shows a slight decline, of about 5 hours, in the number of hours worked by teens in other household businesses.

[Insert Table 5 here]

4.4 Impact on consumption and savings

In this section we analyze whether borrowers' access to credit and the resulting increased engagement in entrepreneurial activities fed through to improved

²⁴Unreported regressions show that there is no strong impact of access to group loans on enterprise ownership by, or jointly with, the borrower's partner. The effect in column 3 is thus driven by an increase in female entrepreneurship.

household well-being - a key objective of the program. To do so, we first estimate the effects of the program on household consumption expenditures and the likelihood of consuming certain items. We use detailed information on consumption patterns elicited in the surveys, in which food consumption is measured over the past week (at a disaggregated level as well as overall) and non-durable and durable consumption over the past month and year, respectively. Results are reported in Table 6.

We find robust evidence that access to group loans led to more (and healthier) food consumption (column 4).²⁵ To put this into context, the average monthly food consumption in group-lending (and control) villages was \$110 per household. The estimated effect implies that food consumption was on average \$17.74 (i.e. 16.1 percent of baseline food consumption) higher per household per month for households in group villages than for households residing in control villages.

A closer inspection of the underlying data reveals that households in particular increased their consumption of milk, bread, and non-alcoholic beverages. With the exception of dairy these effects are not only due to increased home production: we also see treated clients purchasing more.²⁶ We do not find any effects on aggregate consumption, expenditure, and savings variables.²⁷

Our evidence on consumption and business creation somewhat contrasts

²⁵Unreported regression results suggest an increase in the consumption of dairy products and bread in particular.

²⁶The expenditure share of food and non-alcoholic beverages in individual consumption in Mongolia amounted to 35.9 per cent in 2005 (source: World Bank International Comparison Program). According to 2009 FAO data on food supply decomposition by energy value, wheat (bread) accounted for 40 per cent of the average Mongolian's food energy supply (as measured in kcal per capita per day). Together with milk (11 per cent) bread thus constitutes one of the main Mongolian dietary components.

²⁷We are mainly interested in the impact on consumption as a whole rather than impacts on specific food items. However, given the importance of bread and dairy in the Mongolian diet it makes sense to look at these staples separately as well. Yet, when we adjust the p-values for individual outcome variables that are interesting in their own right but plausibly belong to the same family, while taking into account the observed correlation structure between these outcomes -following Aker, Boumnijel, McClelland, and Tierney (2011) and Gibson, McKenzie and Stillman (2013)- most of the individual impacts are no longer statistically significant (with the exception of the impacts on the consumption of non-alcoholic beverages and the ownership of VCRs/radios).

with recent evidence from other microcredit field experiments, such as Banerjee et al. (2013) in India and Augsburg et al. (2012) in Bosnia and Herzegovina, who find that clients who start new businesses reduce consumption, at least in the short run and probably to be able to finance the new business. Our results could be explained by the fact that our follow-up survey is conducted 18 months after the start of the program. About 47 per cent of all borrowers were already in their second loan cycle. This would imply that the women who did start a new business might be already reaping the returns and the higher (permanent) income of such an activity.

We also consider whether the program affected ownership of household goods. We construct an index of six key households goods in the Mongolian context: land-line telephone, mobile telephone, TV, VCR, small electric appliances, and large electric appliances. We find no impact of access to group credit on these household goods (although access to group credit did have an impact on ownership of *business* assets, see Table 3). In contrast, access to individual-liability loans did have a positive impact on the ownership of these household goods (column 8).²⁸

[Insert Table 6 here]

4.5 Social impacts: schooling and informal transfers

Table 7 summarizes the impacts of access to credit on schooling and the labor supply of children aged 6-15 and 16-20 (teens). We do not find any evidence of schooling impacts or of clear effects on child labor. There is some weak evidence at the 10 percent level that among the low-educated group borrowers,

²⁸We do not find a significant increase in the total number of animals, taking the actual number of animals owned or when looking at the number of standardized Mongolian livestock units or *bod* (one horse, yak, or cattle equals one bod; one camel equals 1.4 bod; one sheep equals 1/6 bod; and one goat equals 1/7 bod).

where our main impacts so far have been concentrated, there is some substitution away from outside labor by children to helping out in the newly established female-run enterprises. For teenagers we find positive impacts on schooling among the higher-educated households, both in the group and the individual lending villages.

Our results so far paint a different picture of the impact of the program in group and individual villages, with evidence that the group loans were relatively more effective at achieving their objectives of increasing entrepreneurial activities and improving household well-being. Since we do not find an accompanying effect on household income, an interesting question is the extent to which interpersonal transfers are affected by the programs, and whether they are affected differently in group and individual villages.

As in many other developing countries, access to informal credit/transfers from friends and family is important in Mongolia, in particular for women (National Statistics Office, 2006). Kinship and social networks are confined to relatively small groups of people as they derive from the traditional *khot ail* support system in which a limited number of nomadic households traveled, camped, and herded together for one or more seasons (Enkhamgalen, 1995). Within *khot ail* and similar social networks rural Mongolians often share income from entrepreneurial activities as well as pensions and other allowances.

Access to formal credit may have changed informal lending and transfer behavior in two ways. On the one hand, the increased availability of formal credit in treatment villages may have strengthened informal support networks as additional funds could be shared. On the other hand, informal networks may have weakened as borrowers substitute formal for informal credit, thereby crowding out insurance systems based on implicit reciprocal agreements.

Our survey asked households about their informal - monetary and in-kind - transactions with friends and family during the past year and the most recent month. The right-hand side of Table 7 shows that we do not find an overall

ITT effect of either lending program on the probability of informal transfers taking place (columns 6 and 8). On average about 40 (59) percent of our control respondents received (made) informal transfers to family members and friends. Columns 7 and 9 show, however, that the amounts received are higher in the individual villages, whereas the impacts in the group-villages are consistently negative.

[Insert Table 7 here]

Figure 1 depicts the relationship between the intensity of exposure to credit and the probability of receiving or giving transfers. The difference between the intensity effects on transfers between both types of treatment villages is always significant at the 1 or 5 percent level. The graphs show the probability of making (left) or receiving (right) informal transfers to and from friends for an average respondent in the group-lending (top) and individual-lending villages (bottom) as a function of the average number of XacBank loans taken by respondents in the village. These results are suggestive of group borrowers partly substituting their informal networks with the formal network of the borrowing group. The associated discipline may make them less amenable to use part of their loans to help friends and family smooth consumption. In contrast, individual borrowers increase their informal financial transactions with friends and family, perhaps using part of their new loan to help others out.

Such an interpretation would be in line with recent evidence for Sri Lanka and Ghana by De Mel, McKenzie and Woodruff (2009) and Fafchamps, McKenzie, Quinn, and Woodruff (2011), respectively. The latter paper finds that women who received cash transfers did not increase their business profits as large portions of the cash grants ended up in household consumption and, to a lesser extent, transfers to others. Self-control problems, i.e. borrowers' inability to commit themselves to invest large parts of the cash grants into their enterprises

and to resist the temptation to spend money on competing demands, including from friends and family, were a core explanation for the ineffectiveness of cash grants. Our results are also in line with Karlan and Zinman (2011), who find that individual-liability loans may increase access to informal credit from friends and family in the case of emergencies.

[Insert Figure 1 here]

5 Risk and loan take-up

In this section, we look at the relationship between risk and the take up of loans. We investigate the hypothesis that the larger impacts of the joint-liability loans we document in Section 4 could be explained by the fact that these lending contracts might provide a way to share the risk of taking up a loan. We start by presenting a simple theoretical framework to interpret our empirical results. In particular, we want to stress the difference between joint and individual-liability contracts in terms of risk sharing by borrowers. Hence, unlike the early papers in the microfinance literature, the emphasis is not so much on repayments but on the take-up of productive investments. We do not discuss specific credit-market frictions, such as moral hazard and adverse selection, that prevent the financing of these projects in the absence of a micro-lender. The main idea that we want to outline is that joint liability might encourage (or provide an institutional setting that allows) risk sharing among group members as, *de facto*, it reduces the amount of risk involved in a given project. Joint liability may therefore lead to an increase in the proportion of women that start a business. This idea and mechanism is consistent with Fischer (2013) and Giné et al. (2011).

To make this simple point we use a static model where individuals choose whether to take up a loan for a risky project or pursue a safe project. While the

model is static, we also consider a term that represents future welfare. In particular, we assume that default implies the loss of future loans from the MFI. As in Giné et al. (2011) the risky project is socially desirable since it has an expected return that is (considerably) higher than the return on the safe project. Whether the excess return compensates for the additional return variability depends on individual risk aversion. Moreover, the availability of joint-liability loans institutionalizes a risk-sharing mechanism that may not be available in the absence of this type of loans.

The simple model we outline below also allows us to interpret a new set of empirical results that we derive using some unique features of our data set. In particular, as we elicit information from our respondents about the subjective probability distribution of the projects they might undertake with the loan (and these questions are asked also of the women who do not take up a loan), we can relate differences in loan take up between individual and joint-liability schemes to the (perceived) riskness of the projects available in different villages. These results can then be compared with the predictions of our simple model and can also give an empirical characterization of results that are ambiguous from a theoretical point of view.

5.1 Theoretical framework

Let's consider individuals who can either invest in a productive but risky project or in a safe project. Each individual is endowed with a quantity D , which may be different across individuals. If invested in the safe project, D provides a certain return of DR^s . We can think of this as financing consumption or some very simple activity that is not particularly risky or productive.

All individuals also have access to a risky project. This risky project requires an investment of $D + B$, where $D \ll B$. We assume that investing in the risky project earns a return R^H with probability q and R^L with probability $1 - q$. There is an MFI that supplies loans of size B to finance the project at a rate R^b . We

assume the following set of inequalities between the various rates

$$R^H > R^s; \quad (3)$$

$$R^b > R^L \geq 0; \quad (4)$$

$$R^H > 2R^b; \quad (5)$$

$$qR^H + (1 - q)R^L > R^k, k = s, b. \quad (6)$$

The first inequality simply says that the return on the risky project, when successful, is greater than the return on the safe project. The second inequality indicates that the borrowing rate is greater than the return on the risky project when it is unsuccessful, while the third inequality says that the return on the risky project is potentially very high. Moreover, this inequality guarantees that in the joint-liability case, a successful partner can bail out an unsuccessful one. Finally, the fourth inequality conveys that the expected value of the risky project is higher than both the return on the safe project and the borrowing rate.

We now need to discuss what happens when an individual has a loan with the MFI and defaults. Although the model is static (we consider only one period) in reality the relationship between the borrower and the MFI may be of a long-term nature. Default may jeopardize this long-term lending relationship and thus involve a cost. We assume that this relationship is valued at some exogenous amount K . One could develop a more complete and coherent model but for the main point we want to make this is not necessary (Giné et al. (2011) use a similar strategy).

At the end of the period individuals enjoy utility that will depend on whether they chose the safe or the risky project and, in the latter case, on whether the project was successful or not. If they do not default on a loan (either because they simply did not borrow or because they borrowed and did not default) they also obtain K utils, which represent the availability of future loans and/or the

reputation with the MFI.

The individuals in our economy are heterogeneous in R^s . We need this heterogeneity to allow for some individuals borrowing while others do not. Heterogeneity in R^s represents differences in the returns on the safe project, such as home production. Albeit potentially different, we assume that all R^s satisfy inequality (6). For expositional simplicity we assume that there are two groups of individuals of the same size in the economy. Within each group the product DR^s is uniformly distributed over the same support. All individuals share the same utility function, which is increasing and concave, as well as the utils that they derive from the long-term relationship with the MFI.

We consider two types of contracts: individual liability and joint liability. The latter is assumed to include, for simplicity, only two individuals. We will further assume that individuals form groups with individuals with similar R^s . This assumption is made to simplify the exposition.

Individual-liability contract

When an individual does not take up a loan, she will retain the long-term relationship lending with the MFI. When an individual-liability loan is taken and the project turns out to be successful, the borrower will repay BR^b and preserve her long-term relationship. However, if the project is unsuccessful, she repays only $(D+B)R^L$, loses the long-term relationship with the MFI, and consumes an exogenous amount y , available to all and that cannot be garnished by the MFI.

Joint-liability contract

In the case of joint liability, we assume that the two borrowers are identical in the product DR^s . That is, we assume that individuals are matched in groups with similar individuals in the one dimension of heterogeneity that we are considering. This assumption simplifies the analysis and it is not crucial for the point we

want to make.²⁹ We assume that the outcomes of the two projects are independent. Hence both projects will be successful with probability q^2 , they will both be unsuccessful with probability $(1 - q)^2$, and only one will be successful with probability $2q(1 - q)$.

Two individuals in a joint-liability group are jointly liable for their loans. As they are identical, they will make the same choices. If they choose the safe project and do not get a loan, they will both receive DR^s and preserve the long-term relationship with the MFI. If they get a loan and both their projects are successful they will both repay their loan and interest and preserve the lending relationship. If the first individual's project is successful and the second is not, the first individual will bail out the second and pay the MFI $2R^bD$ to cover both loans. However, we assume she will get the proceedings of the investment from the second individual $((D + B)R^L)$ as a partial compensation. Symmetrically, if the first individual's project fails while the second's is successful, she will consume only y but preserve the long-term relationship with the MFI as she will be bailed out by her partner.

Results

Table T1 below summarizes the two contractual arrangements described above. We report in each row the utilities obtained by individual 1 in a given scenario. In the columns we report the different contractual arrangements. The table also defines implicitly the quantities k_1, k_2 , and k_3 , which are the consumption equivalents of the utils K . Notice that concavity of the utility function implies that $k_3 < k_2 < k_1$.

²⁹We could have considered different degrees of risk aversion and obtained similar results. We note that in our experiment matching was endogenous and take-up was high, suggesting that assortative matching took place. We have some suggestive evidence that individuals within a group are indeed characterized by similar risk attitudes (see Appendix 1). This evidence is also consistent with that reported in Attanasio et al. (2012) and which suggests that the assumptions underlying our model are not too strong.

Table T1: Utility of individual 1 when the risky project is chosen			
	prob.	under individual liability	under joint liability
both succeed	q^2	$U(y + Y^H) + K =$ $U(y + Y^H + k_1)$	$U(y) + K =$ $U(y + k_3)$
1 succeeds, 2 fails	$q(1 - q)$	$U(y + Y^H) + K =$ $U(y + Y^H + k_1)$	$U(y + Y^H - Y^P) + K =$ $U(y + Y^H - Y^P + k_2)$
1 fails, 2 succeeds	$q(1 - q)$	$U(y)$	$U(y) + K =$ $U(y + k_3)$
both fail	$(1 - q)^2$	$U(y)$	$U(y)$
where $Y^H = DR^H + B(R^H - R^b)$; $Y^P = BR^b - (D + B)R^L$			

Each individual will compute the expected utility under the risky project and compare it to the utility that she can get by investing in the safe project. Which of the two is chosen depends obviously on all the parameters of the model. However, the following proposition is immediate.

Proposition 1. Under individual liability, there exist a level of returns to the safe project R^s , R^{s} , such that individuals with $R^s > R^{s*}$ will invest in the safe projects and individuals with $R^s \leq R^{s*}$ will get a loan from the MFI. Analogously, under joint liability, there is a level of returns to the safe project R^s , R^{s**} , such that individuals with $R^s > R^{s**}$ will invest in the safe projects and individuals with $R^s \leq R^{s**}$ will form groups that take a loan from the MFI.*

The proof of the proposition follows from the consideration of the expected utility under the two alternatives and from the fact that the excess return on the risky project is monotonic and decreasing in R^s .

For future reference it is worth noting that R^{s*} is implicitly defined by the following equation:

$$U(y + R^s * D) = qU(y + Y^H + k_1) + (1 - q)U(y) \quad (7)$$

Analogously, in the case of joint liability, R^{s**} is defined by :

$$U(y + R^{s**} * D) = q^2 U(y + Y^H + k_1) + q(1 - q) U(y + Y^H - Y^P + k_2) \\ + q(1 - q) U(y + k_3) + (1 - q) U(y) \quad (8)$$

Writing the components of expected utility under the risky project in terms of the consumption equivalents k_1, k_2 , and k_3 is useful because it clarifies that the variance of the 'returns' on the risky project decreases under joint liability. This can be seen because under individual liability the probability mass is divided between two points. Under joint liability some of this probability is shifted to points in between these two points.

In this context, for a given utility function, investment choices are determined by the entire return distribution and not just by its variance. Participation in a joint-liability scheme, for instance, might reduce risk but also change the mean of the distribution because the successful individual has to bail out a possibly unsuccessful partner and, in return, is bailed out by her partner in certain states of the world. The overall effect depends on how much individuals value the long-term relationship with the MFI. If K is large enough, the mean will increase and the proportion of individuals that will take a joint-liability loan increases too. An interesting case to consider, and one that delivers a sufficient condition for the increase in the proportion of loan takers under joint liability for a generic *concave* utility function, is the case in which K is such that the 'mean expected return' does not decrease when moving to a joint-liability contract.

Assumption 1. Suppose that K is such that:

$$k_1 \leq (k_2 + k_3 - BR^b + (D + B)R^L) \quad (9)$$

This assumption easily delivers the following proposition.

Proposition 2. Under Assumption 1, the fraction of individuals that takes a loan under a joint-liability contract of the type described in Table T1 is larger than the same fraction under individual liability.

Proof. Assumption 1 implies that:

$$\begin{aligned}
q(y + DR^H + B(R^H - R^b) + k_1) + (1 - q)y &\leq q^2(y + DR^H + B(R^H - R^b) + k_1) \\
&+ q(1 - q)[y + (D + B)R^H - 2BR^b + (D + B)R^L + k_2] \\
&+ q(1 - q)[y + k_3] + (1 - q)^2y
\end{aligned} \tag{10}$$

The left-hand side of this expression is the expected return under individual liability, while the right-hand side is the expected return under joint liability. Proposition 2 then follows from the concavity of the utility function. $\square$

The intuition behind the proposition is quite simple. A joint-liability contract does two things. On the one hand it offers some insurance as the two agents can share risk. On the other hand, there is a change in the mean return driven by two considerations. First, if the individual risky project is unsuccessful, the individual will be able to retain the relationship with the bank. This is valued at K . Second, she has to bail-out her partner in the event she is successful and the partner is unsuccessful. This is valued at $Y^P = BR^b - (D + B)R^L$. Thus, when K is relatively high or Y^P relatively low, an individual is more likely to value joint liability and the assumption of the proposition holds. In this case the insurance effect will prevail. Notice that Assumption 1 is a sufficient but not a necessary condition. There might be values of K that violate Assumption 1 and yet a larger number of individuals take up the loan under joint than under individual liability.

If we define $Y^H \equiv DR^H + B(R^H - R^b)$, as we did in Table T1, then expected

income of the risky project under individual liability (including the consumption value of the reputation with the bank) is $q(Y^H + k_1)$ while the variance of the risky project income is $q(1 - q)(Y^H + k_1)^2$. We now want to assess what happens with the fraction of individuals taking up a loan when the riskiness of the project increases. To do so, we consider mean preserving spreads of the return distribution. One way of doing this is to increase $q(1 - q)$ but changing Y^H so that $q(Y^H + k_1)$ stays constant. This implies that, if we want to keep the mean of the return on the risky project at some value $\bar{\omega} = q(Y^H + k_1) > R^j, j = s, b$; we will have that as we change q , $Y^H + k_1 = \frac{\bar{\omega}}{q}$. The variance, along this set of parameters, will be given by $\frac{\bar{\omega}^2(1-q)}{q}$, which is decreasing in q . Analogously, in the case of joint liability, one would have to adjust the parameters of the relevant distribution to keep the mean constant as the variance changes.

Proposition 3. Under individual liability, the fraction of individuals getting a loan is decreasing in the variance of the risky project, for a given mean of the risky project. Under joint liability, the fraction of individuals getting a loan is a non-increasing function of the variance of the risky project for a given mean.

Proof. The proposition follows from the definition of R^{s*} and R^{s**} in equation (7) and (8). As the mean of the income prospect does not change but the variance increases, the right-hand side of this expression will decrease, by concavity of the utility function. Therefore, under individual liability, R^{s*} will decrease and, as a consequence, the proportion of individuals taking up a loan will decrease. Under joint liability, Assumption 1 guarantees that, as we change Y^H to keep the mean of the underlying distribution (relevant for the individual liability case) constant when the variance changes, the level of R^{s**} will have to move in the same direction of R^{s*} . $\square$

An interesting question is whether the impact of changes in the variance on loan take up rates are larger under joint or individual liability. Another way to ask the same question is whether the difference in the proportion of women taking loans varies with the risk of the project. Formally, we would be interested

in how the difference of R^{s*} and R^{s**} changes with the variance of the risky project, that is in signing $\frac{\partial(R^{s**}-R^{s*})}{\partial q}|_{Y^H+k_1=\frac{w}{q}}$. Unfortunately, even in the simple model we are considering, a general result cannot be obtained, as it will depend on the features of the utility function and the magnitude of the various parameters in the model, including the relative size of y, k_1, k_2, k_3 . The answer to this question therefore will be an empirical one.

5.2 Village risk and loan take-up

A unique feature of our data is that it contains information on the subjective probability distribution of the returns on the investment projects that could be financed by the XacBank loans. The survey asked all women (both in treatment and control villages) about the range of variation of the returns to the investment. This information is obtained eliciting the maximum and minimum possible values that future returns can take. In this particular survey, budget constraints prevented us from eliciting additional information on the subjective distribution of future returns, such as different points of the CDF. However, other studies (Attanasio and Di Maro, 2009; Attanasio, Meghir, and Vera-Hernandez, 2005) have shown that the range of variation for a given variable (either in terms of the ratio of the maximum to the minimum value or its log) correlates well with the variance of such a variable, which can be obtained with additional information on the CDF and some assumptions on its functional form. We therefore use the log of the ratio of maximum to minimum return as our proxy for risk. Additional details on the questions about subjective expectations can be found in Appendix 1, which also reports some descriptive statistics on our proxy of village risk.³⁰

We also consider the possibility that this measure of risk can be endoge-

³⁰Table A3 in the Appendix shows that perceived income uncertainty is higher in villages that recently experienced a crop disaster; where more people lost their job over the last year; with less informal inter-household transfers (and hence less informal insurance mechanisms); where more children need to be fed; where dairy production is one of the main local industries (vulnerable to weather shocks); and where a lower percentage of the population is Buddhist (indicating a more heterogeneous ethnic composition as Buddhism is the main religion in Mongolia).

neous, in that it refers to specific projects that individuals choose. Moreover, because of its nature, our measure is likely to be affected by measurement error. To circumvent these problems, rather than using the value of individual risk measures, we take averages at the village level. This is equivalent to using village dummies as an instrument for this variable.

As outlined in Section 4, we find that access to group loans was relatively more effective in increasing entrepreneurial activities and improving household well-being. We also find that loan take up was significantly higher in the group-lending as compared to the individual-lending villages. The model in Section 5.1 suggests that the riskiness of available projects in a village may be an important underlying factor. To further explore whether local risks are indeed driving the higher loan take up in the group-lending villages, Table 8 displays the results of a probit model, estimated at the individual respondent level, to explain the probability of loan take-up.

[Insert Table 8 here]

Columns 1 to 3 confirm that in group-lending villages the probability of borrowing from XacBank is higher than in individual-lending villages, by about 13 percentage points (significant at the 10 percent level) as predicted by Proposition 2. This result holds also when we control for a set of borrower covariates and include shock indicators and province fixed effects (see columns 2 and 3). Most (unreported) household-level covariates are not strongly correlated with loan take-up, although we find that households that own a well, fence, or tools and machinery had a higher probability of getting a loan, either because they are more wealthy or because they could use these items as collateral. Otherwise, group and individual borrowers are observationally very similar. They also do not differ in terms of risk aversion (p-value: 0.33) or subjective income risk (p-value: 0.21), which we test in the context of assortative matching (see

Appendix 1). Once again, this confirms that our treatment groups are essentially balanced.

We next explore whether the higher loan take up in group villages can be explained by risk-diversification motives, as our model would suggest. In columns 4 to 6 we add as a measure of village risk the village average of the log difference between respondents' maximum and minimum project returns. The higher this measure the more uncertain villagers are about the returns on potential investments. Note that in all three columns we also control for average expected village income, to take into account that Propositions 2 and 3 in Section 5.1 keeping average return fixed.

In line with Proposition 3, we find that loan take up is significantly lower in more risky villages where return uncertainty is higher. Women that are more uncertain about future returns will be less willing to commit to the fixed repayment schedule associated with a loan. Whilst the coefficient on the joint liability dummy is now negative and significant, as the coefficient on the interaction between the same dummy and the risk variable is positive, the average effect of joint liability is positive (for all values of risk observed in the sample).

In "safe" villages, relatively many women may have been disappointed when the randomization meant that they were offered the (relatively burdensome) group loans. Take-up was consequently quite low. In more risky villages, however, group loans were actually quite popular, presumably because in this type of village the benefits of mutual self-insurance -in line with our model- outweighed the (monitoring) costs associated with the joint liability.

5.3 Repayment

In the previous sections, we documented a positive impact of access to group loans on business activities and consumption as well as some weaker effects of access to individual loans. In this section, we analyze the repayment behavior of both types of borrowers. Giné and Karlan (2010) also compare repayment rates

between group and individual lending programs - both with mandatory weekly repayment meetings - and find no significant differences. In contrast, Carpena, Vole, Shapiro, and Zia (2013) find that joint liability is associated with better loan repayment.

To construct our repayment data we use monthly reporting files that XacBank compiled on the basis of its administrative software. These files contain for each borrower the loan amount, interest rate, disbursement and due dates, loan purpose, collateral, overdue principal and interest, paid penalties as well as whether the client defaulted on the loan (defined as customers that were at least 90 days late in repaying one or more loan installments).

Table 9 presents probit regressions to explain the probability of loan default. The dependent variable is a dummy that indicates whether a borrower defaulted ('1') or not ('0'). The first three columns are based on a sample of first-time XacBank loans disbursed as part of the experiment whereas the last three columns are based on the full sample, i.e. including repeat loans to the experiment participants.

[Insert Table 9 here]

We find, regardless of whether we control for borrower and loan characteristics, no significant difference between the probability of default in group- and individual-lending villages. This confirms the findings of Giné and Karlan (2010) although in our case *neither* program included mandatory repayment meetings whereas in their experiment *both* programs included such meetings.

The covariates in columns 2 and 4 give additional information on the borrower and loan characteristics that influence default probability. While the size of the loan does not influence the likelihood of repayment, there is a negative impact (at the 10 percent significance level) of the amount of outstanding debt at the time of the baseline survey on the likelihood of default.

Respondents with outstanding debt at baseline were thus *more* likely to (be able to) repay the subsequent XacBank loan. This suggests that borrowers that had already successfully passed the screening of another lender, were less risky compared with first-time borrowers.³¹ In a similar vein, column (6) indicates that also repeat borrowers were significantly less risky in terms of default, possibly because they had already successfully passed XacBank's own screening procedures and subsequently paid on time. For both first-time and repeat loans we also find that as loans mature (increasing number of months since disbursement) the risk of default increases, all else equal (see also Carpena et al., 2013).

Interestingly, a number of covariates are only of importance for first-time loans. Those that owned land or operated an enterprise at baseline were less risky borrowers as were the relatively highly educated. Ownership of a TV at baseline increased the risk of default, perhaps because this identifies women who use(d) debt for consumptive purposes. None of these variables is statistically significant at the 5 percent level in the regression based on the whole loan sample (column 6). For repeat borrowers these variables are less important compared to the information that is contained in the variable that measures the number of successful previous loans with XacBank during the experiment.

Lastly, in unreported regressions we look at interaction effects between the liability structure and the number of previous loans. We find no evidence for a differentiated impact of repeat borrowing under the two programs. We also try other interaction terms but none of these is statistically significant, implying that there is no apparent heterogeneity between group and individual borrowers in terms of repayment behavior.

³¹To the extent that multiple borrowing and over-indebtedness were a problem in rural Mongolia this is therefore not picked up by our default analysis. The fact that we do not find differences in repayments rates does not imply, however, that borrowers with initial debt did not experience any difficulties; it just shows that in the end they managed to repay as well as first-time borrowers. High repayment rates can point to successful projects with high returns but may also mask underlying problems where borrowers need to borrow from other sources or sell assets in order to repay.

6 Conclusions

We present results from a randomized field experiment in rural Mongolia where group-lending and individual-lending programs were randomly introduced across villages. The aim of the study was to measure and compare the effectiveness of these two types of microcredit in reducing poverty – a topic that still lacks unequivocal evidence, in particular for rural settings. While earlier papers have separately assessed the poverty impact of group lending (Banerjee et al., 2013) and individual lending (Karlan and Zinman, 2011) this is the first field experiment to compare both in the same (rural) setting.

Our findings on the poverty impact of different modes of microcredit are mixed. In line with previous studies, we document that participants in both programs used part of their loans to acquire assets – VCRs, radios, and large household appliances. A second finding that holds for the group-lending program in particular is that women with lower education seem to benefit more from the intervention than women with higher education. We interpret the level of education as a proxy poverty measure, more reliable than a wealth indicator given that it is not affected by the program and is more stable over time. The results therefore suggest that it is the poorer part of the targeted population that benefits more from this microcredit intervention.

For group loans we also find a positive impact on food consumption and entrepreneurship though not on current income. Among households that were offered group loans the likelihood of owning an enterprise increases by ten percentage points more than in control villages (and even by 30 percentage points for less-educated women). Our findings for individual lending are much weaker and it is unlikely that these activities will ultimately have the same impact as those of group clients.

Importantly, we find no difference in repayment rates between the two lending programs, both of which did not include weekly repayment meetings. This

casts doubt on the hypothesis that microcredit repayment rates are high mainly due to the effect of weekly group meetings. Our results indicate that, at least in our context, even without such regular meetings group and individual microcredit can have similar and high repayment rates (also note that both our loan products required some form of collateral).

If one were to take at face value the evidence on the stronger impact of group loans, one would want to ask *why* such loans are more effective at raising consumption (and probably long-term income) than individual loans. One possibility is that the joint-liability scheme better ensures discipline so that larger long-run effects can be achieved. Group borrowing may foster self-discipline and in doing so ensure that a substantial part of the loans is actually invested in the first place (instead of used for consumption or transfers to others).³² Our results on informal transfers can be interpreted to support this hypothesis: women in group-lending villages decrease their transfer activities with families and friends, opposite to what we find in individual-lending villages. This could reflect that groups replace some of their informal financial networks but further analysis is needed to explore this. Such an analysis would also be important to assess the welfare impact of access to group loans for the borrowers as well as their friends and families. Increased within-group financial discipline may come at the cost of disrupting informal credit and insurance systems based on kinship and other social ties.

We also provide a simple model and empirical evidence to suggest that joint-liability loans may be better suited to set up a new business in a relatively uncertain environment. Some women, in particular the less-educated, may not have been comfortable with borrowing on an individual basis but *were* willing

³²Informal follow-up interviews with a number of group borrowers indicated that many valued the upfront joint-savings requirement of group loans. This would be in line with recent findings by Bauer, Chytilová, and Morduch (2012) who show for a population of Indian borrowers that women with present-biased preferences, who had difficulties saving on their own due to a lack of self-discipline, were particularly likely to borrow via self-help groups (a crucial element of which was the joint accumulation of savings).

to borrow as part of a group when XacBank made that option available in their village. This would imply that group and individual lending are complementary financial services for which the demand may differ across borrower types and across different risk environments. The continuing process of liability individualization by MFIs may therefore run the risk that certain borrowers, those that are not able or willing to borrow and invest on their own, may gradually lose access to formal financial services.

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Appendix Tables and Figures

[Insert Tables A1-A2-A3 and Figures A1-A2 here]

Appendix 1 Assortative matching

In this appendix we assess whether group formation in the group-lending villages was characterized by assortative matching. In these villages an average of fifteen women borrowed as part of a joint-liability group and the question at hand is whether they self-selected into relatively homogenous risk groups. To analyze this issue we first create borrower-level measures of both risk aversion and subjective income risk. To measure risk aversion, we count the number of ‘safe’ choices that a borrower picked when going through a set of five paired lottery choices (cf. Holt and Laury, 2002). To measure borrower risk, we elicited information on the distribution of subjective income expectations by asking women about the maximum (minimum) amount of entrepreneurial revenues that they expected to earn in case the next 12 months would be extremely successful (unsuccessful).³³ We then define borrower risk as the log difference between the respondent’s highest and lowest income estimate for the next year.

Next, we used these two risk measures to assess whether the observed borrower groups were more homogeneous in terms of risk aversion and borrower risk when compared to all hypothetical borrower configurations (of the same group sizes) that could have been possible in a village but did not materialize in reality (see also Figure A3). To compare the observed configuration with this universe of hypothetical group configurations, we calculate for each configuration the village-level summary measure *Between*. We do this for both the risk aversion and the borrower risk measure. *Between* is the ratio of the between-group to the overall variance in a village. As a measure of group homogeneity, high values of *Between* indicate that within-group variance is low.

We then compare how homogeneous the observed grouping of borrowers in a specific village is compared to the possible groupings of the same borrowers

³³See for instance Attanasio, Meghir, and Vera-Hernandez (2005) and Delavande, Giné, and McKenzie (2011).

in that village. Due to the limited number of hypothetical groupings in a village it is not possible to calculate the exact percentage of possible groupings that would have resulted in a lower *Between* measure. Instead we calculate a sorting percentile range and report its mean.

For the risk aversion measure, the average village has a mean sorting percentile of 0.62. This means that, on average, 62 per cent of all the possible groupings in the group-lending villages would have resulted in a lower *Between* value than the one observed. That is, the average village is more homogeneous than 62 per cent of all possible groupings. The median village is more homogeneous than 72 per cent of all possible groupings. For the income risk measure, we find similar results. Here the mean (median) sorting percentile is 65 (70) per cent. We conclude that it is likely that the endogenous group formation in the fifteen group-lending villages was to some extent characterized by assortative matching.

[Insert Figure A3 here]

Table 1. Summary statistics

		Control Group			Individual - Control		Group - Control	
	Obs	Obs	Mean	St. Dev.	Coeff.	p-value	Coeff.	p-value
<u>Panel A. Post-attrition household sample</u>								
<u>Household composition</u>								
# members	961	260	4.888	1.828	-0.048	0.851	0.047	0.712
# adults (>=16 years old)	961	260	1.754	1.255	0.009	0.958	0.005	0.950
# children (<16 years old)	961	260	3.158	1.530	-0.069	0.720	0.032	0.746
Age of respondent	961	260	40.881	9.360	-1.521	0.179	-0.506	0.337
Education of respondent (1=at most grade VII)	961	260	0.150	0.358	-0.013	0.754	-0.021	0.289
Religion of respondent (1=Buddhist)	961	260	0.758	0.429	-0.058	0.375	0.000	0.998
<u>Access to credit:</u>								
Loan from bank	961	260	0.477	0.500	0.100	0.152	0.032	0.364
Loan from relatives	961	260	0.023	0.150	0.005	0.699	-0.002	0.801
Loan from friends	961	260	0.046	0.210	-0.009	0.578	-0.007	0.359
Any other loan	961	260	0.065	0.248	0.035	0.260	0.014	0.549
Any type of loan	961	260	0.573	0.496	0.113	0.066	0.035	0.290
<u>Amount borrowed from ('000s MNT):</u>								
Bank	948	260	362	637	113	0.197	82	0.066
Relatives	942	256	0.5	6	-0.50	0.187	-0.2	0.461
Friends	943	256	1.3	8	-0.59	0.188	-0.4	0.084
Other	941	255	3.3	18	4.29	0.061	2.7	0.252
Total	946	260	389	641	133	0.118	81.3	0.058
<u>Self-employment activities</u>								
Any type of enterprise	961	260	0.60	0.490	-0.012	0.849	0.000	0.998
Respondent has own enterprise	961	260	0.396	0.490	-0.005	0.937	-0.016	0.566
Revenue of respondent's enterprise	961	260	515.0	1,388	-88.81	0.422	5.605	0.940
Expenses of respondent's enterprise	961	260	390.0	966.0	-82.45	0.398	7.711	0.909
Profit of respondent's enterprise	960	260	125.0	898.3	-6.363	0.881	1.085	0.972
Business asset index	961	260	0.03	0.910	0.043	0.808	0.030	0.701
Distance to province center (in km)	937	260	113	52.27	30.26	0.132	-12.04	0.237
<u>Employment activities (except self-employment)</u>								
# of income sources	961	260	0.546	0.742	0.042	0.646	0.062	0.227
Wages from agricultural work	961	260	0.088	0.285	0.037	0.361	0.021	0.227
Wages from private business	961	260	0.119	0.325	0.006	0.834	0.023	0.175
Wages from mining	961	260	0.023	0.150	-0.012	0.517	0.011	0.332
Wages from teaching	961	260	0.112	0.315	-0.009	0.786	-0.014	0.369
Wages from government	961	260	0.100	0.301	0.014	0.737	0.003	0.882
Income from benefits	961	260	0.850	0.358	-0.027	0.457	0.000	0.976
Any other income	961	260	0.073	0.261	0.030	0.412	0.010	0.517
<u>Consumption ('000s MNT)</u>								
Total consumption expenditures (yearly)	929	252	2,800	2,200	-140.00	0.766	6.49	0.979
Durable consumption (yearly)	949	255	710	700	140.00	0.099	84.00	0.067
Non-durables consumption (monthly)	949	259	89.87	110	-6.300	0.601	-1.80	0.779
Food consumption (weekly)	950	257	21.06	22.32	-4.300	0.539	-1.10	0.764
Household asset index	961	260	0.06	0.95	-0.115	0.414	0.00	0.973
<u>Panel B. Attrition</u>								
Not surveyed at endline	1,148	299	0.130	0.337	0.070	0.082	0.008	0.689
Not surveyed at endline (excl. wedding villages)	1,087	299	0.130	0.337	0.050	0.196	0.008	0.689

Notes: Data source: baseline household survey. Unit of observation: household. Panel A: sample includes only households also surveyed at endline. Panel B: sample includes all households surveyed at baseline. In case of household characteristics, the standard errors are clustered at the village level. Table A1 provides the definitions and sources of all variables. *Wages from private business* includes wages from working in a shop, market, bank, finance company, or other private business. *Household (business) asset index*: Calculated for a list of six (four) home electrical appliances (business assets). Stock, not flow. Each asset is given a weight using the coefficients of the first factor of a principal-component analysis. Each index, for a household i , is calculated as the weighted sum of standardized dummies equal to 1 if the household owns the durable good. '000s MNT: Thousands of Mongolian tögrög. The exchange rate at baseline was USD 1 to MNT 1,150.

Table 2. Credit

		(1) XacBank	(2) Other bank or MFI	(3) Informal loans	(4) Any loan	(5) Default XacBank loan (admin data)	(6) Default XacBank loan (survey data)	(7) Delay XacBank repayment (admin data)	(8) Delay XacBank repayment (survey data)
Panel A. Credit access [†]									
Individual treatment		0.436*** (0.065)	-0.080* (0.046)	0.005 (0.007)	0.210*** (0.054)	0.005 (0.004)	0.025 (0.025)	0.040** (0.017)	0.056** (0.024)
By education	Low	0.469*** (0.083)	-0.154** (0.064)	-0.001 (0.001)	0.157* (0.081)	0.014 (0.011)	0.011 (0.034)	0.027 (0.017)	0.053 (0.036)
		0.418*** (0.065)	-0.038 (0.061)	0.007 (0.011)	0.240*** (0.066)	0.000 (0.000)	0.033 (0.022)	0.046** (0.019)	0.059** (0.025)
	High								
Group treatment		0.511*** (0.047)	-0.123** (0.057)	0.010 (0.009)	0.268*** (0.049)	0.000 (0.000)	0.010 (0.016)	0.067** (0.027)	0.073*** (0.022)
By education	Low	0.510*** (0.065)	-0.180** (0.080)	0.019 (0.013)	0.221** (0.086)	0.001 (0.001)	-0.021 (0.022)	0.035* (0.018)	0.042 (0.034)
		0.509*** (0.051)	-0.091 (0.068)	0.006 (0.009)	0.295*** (0.064)	0.000 (0.000)	0.026 (0.016)	0.082** (0.036)	0.089*** (0.028)
	High								
Observations		961	961	961	961	814	961	812	961
Control mean follow-up		0.0615	0.454	0.0039	0.504	0.000	0.0154	0.000	0.0154
Panel B. Loan amounts (in MNT) ^{††}									
Individual treatment		346,776*** (50,076)	-10,630 (82,977)	5,124 (4,324)	342,131*** (49,899)				
By education	Low	409,279*** (78,107)	-8,056 (97,251)	-1,171 (1,015)	404,122*** (78,437)				
		314,66*** (47,017)	-10,346 (110,360)	8,142 (6,778)	310,401*** (46,261)				
	High								
Group treatment		378,366*** (46,539)	-15,040 (109,777)	10,744 (6,380)	373,495*** (46,175)				
By education	Low	428,099*** (81,437)	65,348 (188,178)	16,994 (15,466)	421,919*** (81,840)				
		352,881*** (51,185)	-49,909 (134,880)	7,913* (4,397)	348,774*** (50,534)				
	High								
Observations		961	961	961	961				
Control mean follow-up		37.204	486.436	19.23	53.075				

Notes: Data source: baseline and follow-up household surveys. The upper panel shows the results of difference-in-differences ITT regressions to measure the impact of providing access to group and individual loans on borrowing (upper panel) and borrowing amounts (lower panel). All variables measured at the household level. Within each panel we show the overall impacts for both types of treatment villages ("Individual treatment" and "Group treatment") as well as heterogeneous impacts by the level of education, where "High" education is defined as grade 8 (or higher) or vocational training; and "Low" as less than grade 8. All regressions include a set of unreported pre-treatment covariates. Standard errors are clustered at the village level and reported in parentheses. The exchange rate at baseline was USD 1 to Tögrög 1,150. ***, **, * indicate significance at 1, 5, and 10%. *Delay XacBank repayment*: delayed loans are those that were at least 90 days late at any point in time. Table A1 provides the definitions and sources of all variables.

Table 3. Self-employment activities: revenues, assets and profits

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		All household businesses				Respondent business			
		Assets (stock in '000s MNT)	Business asset index	Has a self- employment activity	No. self- employment activities	Profit ('000s MNT)	Has a self- employment activity	Profit ('000s MNT)	Business started
Individual treatment		-403.0 (595.48)	0.193 (0.183)	-0.009 (0.059)	-0.045 (0.075)	75.26 (86.04)	-0.025 (0.058)	-1.07 (41.35)	-0.023 (0.014)
By education	Low	-372.16 (837.11)	0.304 (0.184)	0.047 (0.069)	-0.099 (0.114)	88.75 (90.12)	0.058 (0.081)	2.90 (45.04)	-0.015 (0.021)
		-401.24 (526.65)	0.134 (0.204)	-0.038 (0.073)	-0.011 (0.079)	63.76 (85.21)	-0.071 (0.057)	-6.316 (41.26)	-0.028 (0.019)
	High								
Group treatment		-895.06 (553.44)	0.145 (0.166)	0.085 (0.054)	0.003 (0.068)	43.21 (99.04)	0.101* (0.058)	-11.68 (60.35)	0.009 (0.020)
By education	Low	-476.49 (822.77)	0.376** (0.180)	0.184*** (0.064)	-0.070 (0.115)	31.13 (100.09)	0.308*** (0.074)	-16.25 (61.62)	0.029 (0.026)
		-1,066.2** (481.62)	0.033 (0.187)	0.037 (0.071)	0.046 (0.072)	42.6 (97.62)	-0.002 (0.060)	-13.60 (60.15)	-0.002 (0.027)
	High								
Observations		961	961	961	961	961	961	961	961
Control mean follow-up		2,236	0.037	0.585	0.331	-26.85	0.392	-12.11	0.0654

Notes: Data source: baseline and follow-up household surveys. Coefficients and standard errors (in parentheses) from OLS difference-in-differences ITT regressions. Columns (1)-(4) are at the household level and columns (5)-(7) at the respondent level. The table shows the overall impacts for both types of treatment villages ("Individual treatment" and "Group treatment") as well as heterogeneous impacts by the level of education, where "High" education is defined as grade 8 or higher (or vocational training) and "Low" as less than grade 8. All regressions include a set of unreported pre-treatment covariates. Standard errors are clustered at the village level. The exchange rate at baseline was USD 1 to Tögrög 1,150. ***, **, * indicate significance at 1, 5, and 10%. *Business asset index*: Calculated for a list of four key business assets: tools and machinery, riding equipment, lorry or tractor, and unsold stock. Stock, not flow. Each asset is given a weight using the coefficients of the first factor of a principal-component analysis. The index, for a household i , is calculated as the weighted sum of standardized dummies equal to 1 if the household owns the durable good. *Business started* = 1 if the respondent at the time of the follow-up survey had a business that was less than 20 months old. Table A1 provides the definitions and sources of all variables.

Table 4. Income

		(1) Self- employment (profit)	(2) Daily labor/salaried	(3) Household benefits	(4) Other
Individual treatment		75 (86)	-245 (259)	2,317 (2,184)	-146 (104)
<i>By education</i>	<i>Low</i>	89 (90)	-137 (198)	1,496 (3,402)	-221 (205)
	<i>High</i>	64 (85)	-303 (327)	2,716 (2,285)	-101 (86)
Group treatment		43 (99)	-341 (274)	2,553 (2,740)	630 (638)
<i>By education</i>	<i>Low</i>	31 (100)	-256 (216)	3,112 (3,397)	1,282 (1,187)
	<i>High</i>	43 (98)	-388 (345)	2,308 (2,929)	347 (373)
Observations		961	961	961	960
Control mean follow-up		-26.85	413.9	12679	506.1

Notes: Data source: baseline and follow-up household surveys. Coefficients and standard errors (in parentheses) from OLS difference-in-differences ITT regressions. The table shows the overall impacts for both types of treatment villages ("Individual treatment" and "Group treatment") as well as heterogeneous impacts by the level of education, where "High" education is defined as grade 8 or higher (or vocational training) and "Low" as less than grade 8. All regressions include a set of unreported pre-treatment covariates. Standard errors are clustered at the village level. The exchange rate at baseline was USD 1 to Tögrög 1,150. ***, **, * indicate significance at 1, 5, and 10%. Table A1 provides the definitions and sources of all variables. All variables expressed in '000s Tögrög. The exchange rate at baseline was USD 1 to Tögrög 1,150.***, **, * indicate significance at 1, 5, and 10%. *Other* includes the value of food produced for self-consumption.

Table 5. Time worked by household (HH) members

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Hours worked per adult member over the past 7 days of age group $\dagger$:												
	all adults and teens				teens				prime age adults			
	total	<i>of which:</i> respondent's business	other HH business	outside activities	total	<i>of which:</i> respondent's business	other HH business	outside activities	total	<i>of which:</i> respondent's business	other HH business	outside activities
Individual treatment	0.913 (9.665)	2.740 (5.317)	-5.060 (6.357)	3.233 (4.347)	1.309 (2.810)	0.729 (1.173)	0.370 (1.360)	0.210 (1.202)	-0.213 (7.812)	1.844 (3.994)	-4.982 (6.108)	2.925 (4.113)
By education												
Low	-4.865 (11.636)	4.816 (5.732)	-14.697* (8.241)	5.016 (4.940)	-0.702 (3.626)	-0.431 (1.630)	-0.435 (1.851)	0.164 (1.506)	-2.844 (8.970)	4.312 (4.244)	-11.937 (7.792)	4.781 (4.484)
High	3.997 (10.993)	1.140 (5.616)	0.326 (7.984)	2.531 (5.056)	2.352 (2.991)	1.284 (1.033)	0.826 (1.486)	0.242 (1.605)	1.210 (9.126)	0.153 (4.453)	-1.176 (7.378)	2.234 (4.663)
Group treatment	-2.605 (9.228)	7.841 (5.240)	-7.560 (5.640)	-2.887 (4.369)	-3.004 (3.069)	-0.087 (1.226)	-3.317* (1.690)	0.401 (1.292)	2.799 (6.871)	7.45* (4.061)	-2.220 (5.050)	-2.426 (3.796)
By education												
Low	-4.345 (12.435)	18.727*** (6.065)	-17.805* (9.761)	-5.268 (5.820)	-5.828 (3.937)	-1.325 (1.711)	-4.592** (2.223)	0.089 (1.559)	5.408 (9.831)	19.16*** (4.672)	-9.352 (8.710)	-4.400 (5.153)
High	-1.413 (10.127)	2.137 (5.351)	-1.928 (6.792)	-1.622 (4.759)	-1.613 (3.182)	0.479 (1.071)	-2.648 (1.654)	0.555 (1.623)	1.811 (7.692)	1.477 (4.379)	1.626 (6.228)	-1.292 (4.324)
Observations	961	961	961	961	961	961	961	961	961	961	961	961
Control mean follow-up	88.41	19.83	37.52	31.06	5.535	1.277	2.396	1.862	80.63	17.72	33.71	29.19

Notes: Data source: baseline and follow-up household survey. Coefficients and standard errors (in parentheses) from an OLS difference-in-differences ITT regression. All variables measured at the household level. Households with no teenage or prime-age adults are coded as having 'zero' hours for these potential HH members. The table shows the overall impacts for both types of treatment village ("Individual treatment" and "Group treatment") as well as heterogeneous impacts by the level of education, where "High" education is defined as grade 8 or higher (or vocational training) and "Low" as less than grade 8. All regressions include a set of unreported pre-treatment covariates. Standard errors are clustered at the village level. ***, **, * indicate significance at 1, 5, and 10%. Teens include all HH members of age 16-20 inclusive. Prime age adults are all HH members older than 20 years. $\dagger$ Includes hours worked on average per adult member in self-employment and outside activities (i.e. housework is excluded). Households were asked during the end line survey about the no. of hours worked by each HH member over the past 7 days.

Table 6. Consumption

		(1) Total per capita	(2) Durables	(3) Non-durables	(4) Food	(5) Education	(6) Temptation goods and entertainment	(7) Savings	(8) Household asset index
Individual treatment		0.040 (0.078)	-0.053 (0.119)	0.016 (0.141)	0.010 (0.069)	0.169 (0.281)	-0.115 (0.211)	-0.379 (0.375)	0.170** (0.071)
<i>By education</i>	<i>Low</i>	0.016 (0.077)	-0.214 (0.145)	0.151 (0.197)	0.044 (0.086)	0.047 (0.380)	-0.095 (0.291)	-0.313 (0.373)	0.136 (0.128)
	<i>High</i>	0.055 (0.091)	0.039 (0.130)	-0.049 (0.173)	-0.011 (0.078)	0.243 (0.273)	-0.114 (0.231)	-0.409 (0.378)	0.192** (0.086)
Group treatment		0.110 (0.079)	-0.075 (0.116)	-0.002 (0.147)	0.164** (0.069)	-0.112 (0.293)	0.093 (0.435)	-0.014 (0.340)	0.109 (0.086)
<i>By education</i>	<i>Low</i>	0.157 (0.100)	-0.061 (0.154)	0.154 (0.174)	0.242*** (0.087)	-0.313 (0.363)	-0.513 (0.327)	-0.056 (0.373)	0.172 (0.147)
	<i>High</i>	0.093 (0.087)	-0.064 (0.120)	-0.076 (0.185)	0.122 (0.075)	-0.000 (0.294)	0.383 (0.568)	0.007 (0.346)	0.086 (0.074)
Observations		961	958	918	958	961	961	961	961
Control mean follow-up		10.95	10.82	10.73	10.34	1.079	1.056	0.018	0.314

Notes: Data source: baseline and follow-up household surveys. Coefficients and standard errors (in parentheses) from OLS difference-in-differences ITT regressions. All variables measured at the household level. The table shows the overall impacts for both types of treatment villages (“Individual treatment” and “Group treatment”) as well as heterogeneous impacts by the level of education, where “High” is defined as grade 8 or higher (or vocational training) and “Low” as less than grade 8. All regressions include a set of unreported pre-treatment covariates. Standard errors are clustered at the village level. ***, **, * indicate significance at 1, 5, and 10%. Columns (1)-(4) show logs of monthly HH expenditures in tögrög. Columns (5)-(7) show amounts scaled by the mean amount for the control group at baseline. (8): log of the stock of household savings. (9): The *Household asset index* is based on a list of six electrical appliances (stock, not flow). Each asset is given a weight using the coefficients of the first factor of a principal-component analysis. The index is calculated as the weighted sum of standardized dummies equal to 1 if a household owns the durable good.

Table 7. Social effects

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
		Share of kids aged 6-15 in school	Hours worked per child aged 6-15 over the past 7 days in:			Share of teenagers (aged 16-20) in school	Received transfers from friends/family	Amount received	Transferred money to friends/family	Amount transferred	
			Respondent self- employment activities	Other HH self- employment activities	Outside employment						
Individual treatment		-0.003 (0.030)	0.166 (0.630)	0.098 (0.465)	-0.448 (1.222)	0.053 (0.041)	0.040 (0.059)	42.852*** (13.269)	0.109 (0.067)	48.071 (42.949)	
By education	Low	-0.030 (0.039)	0.935* (0.525)	0.072 (0.505)	-2.325 (1.727)	-0.017 (0.060)	0.088 (0.076)	46.406*** (13.925)	0.202* (0.106)	67.434 (44.109)	
		High	0.013 (0.041)	-0.297 (0.752)	0.055 (0.486)	0.677 (1.529)	0.083** (0.039)	0.014 (0.061)	42.189*** (13.792)	0.065 (0.068)	37.723 (43.130)
	Group treatment		-0.029 (0.032)	0.483 (0.588)	-0.861 (0.742)	-2.023 (1.345)	0.082* (0.040)	0.003 (0.063)	-19.057 (40.011)	-0.002 (0.053)	-3.334 (41.274)
	By education	Low	-0.012 (0.046)	0.892* (0.501)	-0.956 (0.785)	-3.861* (1.965)	0.010 (0.064)	0.064 (0.089)	-14.783 (39.967)	0.028 (0.091)	2.236 (44.467)
High			-0.034 (0.039)	0.181 (0.739)	-0.885 (0.798)	-0.907 (1.310)	0.110*** (0.036)	-0.029 (0.065)	-19.764 (38.997)	-0.016 (0.056)	-7.222 (40.874)
Observations		961	961	961	961	961	961	961	961	961	
Control mean follow-up		0.705	0.831	0.000	1.419	0.261	0.400	0.0131	0.588	0.196	

Notes: Data source: baseline and follow-up household surveys. Coefficients and standard errors (in parentheses) from OLS difference-in-differences ITT regressions. All variables measured at the household level. The table shows the overall impacts for both types of treatment villages ("Individual treatment" and "Group treatment") as well as heterogeneous impacts by level of education, where "High" is defined as grade 8 or higher (or vocational training) and "Low" as less than grade 8. All regressions include a set of unreported pre-treatment covariates. Standard errors are clustered at the village level. The exchange rate at baseline was USD 1 to Tögrög 1,150.***, **, * indicate significance at 1, 5, and 10%. Columns (6)-(9): Transfers between the household and family or friends over the past year (in '000s tögrög).

Table 8. Village characteristics and loan take-up

	(1)	(2)	(3)	(4)	(5)	(6)
			Loan take-up			
Group village	0.130*	0.135*	0.125*	-0.472**	-0.473**	0.604
	(0.073)	(0.073)	(0.068)	(0.186)	(0.186)	(1.450)
Village risk				-0.888***	-0.894***	-0.950***
				(0.269)	(0.282)	(0.321)
Group village*village risk				0.781**	0.777**	0.887**
				(0.314)	(0.312)	(0.356)
Village income					0.015	0.090
					(0.082)	(0.150)
Group village*village income						-0.100
						(0.173)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Economic shock dummies	No	Yes	Yes	Yes	Yes	Yes
Province fixed effects	No	No	Yes	Yes	Yes	Yes
Observations	830	829	829	829	829	829
Pseudo R-squared	0.05	0.05	0.07	0.09	0.09	0.09

Notes: Data sources: XacBank administrative data (dependent variable) and baseline household survey (controls). Coefficients and standard errors (in parentheses) from probit regressions. All variables measured at the respondent level. Standard errors are clustered at the village level and reported in parentheses.***, **, * indicate significance at 1, 5, and 10%. *Village risk*: Village average of the log difference between respondents' highest and lowest income estimate for the next year. *Village income*: Village average of respondents' log expected income in the next year

Table 9. Determinants of loan default

	(1)	(2)	(3)	(4)	(5)	(6)
	Default dummy					
	First loan			All loans		
Group village	0.0287 (0.398)	-25.46* (15.44)	-15.62 (19.38)	0.289 (0.339)	-21.44** (9.851)	-2.823 (11.96)
Village risk		0.443 (1.108)	0.155 (1.194)		-0.223 (0.789)	-0.666 (1.167)
Group village*village risk		10.11** (4.346)	4.807 (3.907)		10.49*** (2.750)	6.448*** (2.217)
Village income		-0.115 (0.601)	0.220 (0.677)		-0.311 (0.422)	0.448 (0.578)
Group village*village income		1.317 (1.045)	0.885 (1.339)		1.017 (0.697)	-0.169 (0.913)
Loan amount (millions)			-0.676 (0.621)			-0.473 (0.526)
Debt at baseline (millions)			-0.184* (0.105)			-0.189** (0.0886)
No. prior XacBank loans						-0.139*** (0.0406)
Months since disbursement			0.0910*** (0.0223)			0.104*** (0.0224)
Owns land			-0.491** (0.199)			-0.150 (0.195)
Owns TV			1.254** (0.619)			0.150 (0.329)
Ownership any business			-0.506** (0.211)			-0.0918 (0.140)
Education >VIII			-0.918*** (0.301)			-0.368* (0.216)
Education vocational			-0.869** (0.347)			-0.283 (0.224)
Observations	327	327	302	638	638	612
Pseudo R-squared	0.0001	0.1935	0.354	0.0089	0.1797	0.317

Notes: Data sources: XacBank administrative data (dependent variable) and baseline household survey (controls). Coefficients and standard errors (in parentheses) from probit regressions. The following additional covariates were included but are now shown (all insignificant): *Age*, *Age squared*, *Buddhist*, *Household size*, *Hahl*, *Collateral value*, *Married*, *Male adults*, *Female adults*, *Children <16*, *Owns fence*, *Owns dwelling*, *Owns vehicle*, *Saver*, *HH crop disaster*, *HH natural disaster*, *HH death*. Standard errors are clustered at the village level and reported in parentheses. ***, **, * indicate significance at 1, 5, and 10%.

Figure 1 Treatment intensity and informal transfers to friends

Notes: These graphs show the probability of making (left) or receiving (right) informal transfers to and from friends for an average respondent in the group-lending (top) and individual-lending villages (bottom) as a function of the average number of XacBank loans taken by respondents in the village. The blue lines indicate the expected probability while the white lines indicate a 95 percent confidence interval.

Table A1. Variable definitions

Variable name	Description	Standard control variable
<i>Respondent and household (HH) level data (# respondents = 1,148). Source: Baseline survey</i>		
Age	Age in years of respondent	X
Age squared	Age in years of respondent squared	X
Assets (stock)	Total value of assets in ('000s MNT)	
At least one loan	Dummy variable that is '1' if the HH had at least one loan outstanding	
Business asset index	Index of a list of four key business assets: tools and machinery, unsold stock, lorry or tractor, riding equipment (stock, not flow). Each asset is given a weight using the coefficients of the first factor of a principal-component analysis. The index is calculated as the weighted sum of standardized dummies equal to 1 if a household owns the durable good.	
Buddhist	Respondent is of the Buddhist religion	X
Children <16	Number of children in the HH younger than 16 years	X
Collateral value	Estimated market value of the collateral (in 000's MNT)	
Durable consumption	Total value of durable consumption over the last month in logs MNT	
Education respondent	Number of years of education of the respondent	
Education high	Dummy variable that is '1' if the respondent completed grade VIII or higher or vocational	
Education >VIII	Dummy variable that is '1' if the respondent completed grade VIII or higher	X
Education vocational	Dummy variable that is '1' if the respondent completed vocational training	X
Female business	Dummy variable that is '1' if the respondent operates her own business conditional on at least one HH business	
Female adults	Number of female household members aged 16 or older	X
Food consumption	Total food consumption of the household over the last week in logs MNT	
Hahl	Respondent ethnicity is Hahl	X
HH crop disaster	Dummy variable that is '1' if the HH experienced severe crop losses during the previous year	
HH death	Dummy variable that is '1' if the HH experienced death of a HH member in the previous year	
HH illness	Dummy variable that is '1' if at least one HH member experienced a serious illness in the previous year	
HH natural disaster	Dummy variable that is '1' if the HH experienced a natural disaster, e.g. <i>dzud</i> , in the previous year	
HH robbery	Dummy variable that is '1' if the HH experienced a robbery in the previous year	
Hours enterprise labor	Total number of hours worked per week by all household members in the respondent's enterprise	
Hours hired	Average number of hours worked per week in peak season by non-HH members in the respondent's enterprise	
Hours wage labor	Total number of hours worked per week by all household members outside the own business in return for a formal wage	
Household asset index	Index of a list of six electrical appliances: land-line telephone, mobile telephone, TV, VCR, small electric appliances, large electric appliances (stock, not flow). Each asset is given a weight using the coefficients of the first factor of a principal-component analysis. The index is calculated as the weighted sum of standardized dummies equal to 1 if a household owns the durable good.	
Household size	Number of children and adults in the household	
Joint enterprise	Dummy variable that is '1' if the respondent operates an enterprise together with her spouse	
Loan amount	Loan amount (in 000's MNT)	
Loans at baseline	Dummy variable that is '1' if the HH had at least one loan outstanding at the time of the baseline interview	X
Male adults	Number of male household members aged 16 or older	X
Married	Dummy variable that is '1' if the respondent is married or living together with partner	X
Months since disbursement	Number of months since the loan was disbursed	
Non-durable consumption	Total value of non-durable consumption over the last month in logs MNT	
No. prior XacBank loans	Number of prior XacBank loans taken by the HH as part of the experiment	
Ownership any business	Dummy variable that is '1' if the HH operates at least one business	
Ownership female business	Dummy variable that is '1' if the respondent operates at least one business herself	
Ownership partner enterprise	Dummy variable that is '1' if the respondent's spouse operates an enterprise but not jointly with the respondent	
Outstanding loans	Number of loans taken by the HH that are still outstanding, conditional on at least one loan outstanding	
Profit any business	Total profits of all household business activities over the last year	
Profit female business	Total profits of respondent-owned business activities over the last year	
Total consumption	Value of total household consumption over the last year in logs MNT	
Total income	Total annual income of the household in the previous year in logs MNT	
Transfers given family	Value of monetary and in-kind transfers given in last 12 months to relatives (in 000's MNT) conditional on giving	
Transfers received family	Value of monetary and in-kind transfers received in last 12 months from relatives (in 000's MNT) conditional on receipt	
Transfers given friends	Value of monetary and in-kind transfers given in last 12 months to non-relatives (in 000's MNT) conditional on giving	
Transfers received friends	Value of monetary and in-kind transfers received in last 12 months from non-relatives (in 000's MNT) conditional on receipt	
Wage earnings	Average weekly wage earnings for wage earners (in 000's MNT)	
<i>Village-level data (# villages = 40). Source: Village survey in Spring 2008 and baseline survey</i>		
Buddhist %	Percentage Buddhist households in the village	
Crop disaster %	Percentage of households in the village that experienced a crop disaster in the previous year	
Dairy village	Dummy variable that is '1' if dairy production is among the three main business activities in the village	
Death %	Percentage of households in the village that experienced a death in the previous year	
Distance to paved road	Distance (in km) from the village to the nearest paved road	
Distance to province center	Distance (in km) from the village to the province center	
District area	Total surface area of the district in km ²	
Doctors in district	Number of doctors in the district (including the village)	
Felt village	Dummy variable that is '1' if felt production is among the three main business activities in the village	

Households in district	Number of households (nuclear families) living in the district (including the village)
Households in village	Number of households (nuclear families) living in the village
Illness %	Percentage of households in the village that experienced an illness in the previous year
Job loss %	Percentage of households in the village that experienced a job loss in the previous year
Livestock in district	Number of livestock (cattle, camels, horses, sheep, goats) in the district (including the village)
Money transfers %	Percentage of households in the village that engaged in money transfers (receiving or giving)
Number of loans	Average number of loans received by the respondents in a village
Number of months	Average number of months between the date when respondents in a village received the first loan and the follow-up survey
Over 60	Average number of household members over 60 in the village
People in district	Number of people living in the district (including the village)
People in village	Number of people living in a village
Price bread	Price of a loaf of bread (in MNT)
Price milk	Price of a litre of milk (in MNT)
Primary schools district	Number of primary schools in district (including the village)
Robbery %	Percentage of households in the village that experienced a robbery in the previous year
SCCs in district	Number of Savings and Credit Cooperatives in the district (including the village)
Secondary school teachers	Number of secondary school teachers in the district (including the village)
Time to paved road	Time (in minutes) to travel from the village to the nearest paved road by car or motorcycle
Time to province center	Time (in minutes) to travel from the village to the province center by car or motorcycle
Under 16	Average number of household members under 16 in the village
Village income	Village average of respondents' log expected income in the next year
Village risk	Village average of the log difference between respondents' highest and lowest income estimate for the next year

Notes: This table provides the names and definitions of the variables used in the empirical analysis in alphabetical order. MNT= Mongolian tögrög.

Table A2 The loan products and their use

Progressive traits	Larger loans, lower interest rate, and longer maturity after each successful repaid loan	
Monthly interest rate	1.5 to 2 percent	
Grace period	One or two months depending on loan maturity	
Repayment frequency	Monthly, no public repayment meetings. In case of group loans, the group leader collects and hands over repayments to the loan officer	
	Individual loans	Group loans
Liability structure	Individual liability	Joint liability
Collateral	Yes but flexible approach	Joint savings (20 percent of loan) sometimes supplemented by assets
Average maturity 1 st loan	224 days	199 days
Average maturity 2 nd loan	234 days	243 days
Average size 1 st loan	\$411	\$279
Average size 2 nd loan	\$472	\$386
Percentage of 1 st (2 nd) loans that are mainly used for:		
- Other business expenses	51 (47)	57 (37)
- Other household expenses	28 (19)	28 (22)
- Mixed expenses	12 (8)	14 (17)
- Education	8 (7)	6 (6)
- Purchase tools/machinery	6 (3)	6 (1)
- Purchase livestock	9 (2)	4 (5)

Notes: This table describes the main characteristics of the individual and group loans. Average loan size is conditional on having a loan. Average loan size of group loans refers to loans per borrower not per group. Loans were disbursed in tögrög not USD. Source of data on maturities and loan size: XacBank. Source of data on loan use: follow-up survey (borrowers could indicate multiple loan purposes).

Table A3. Determinants of village risk

Dependent variable:	Village risk
Crop disaster %	0.409** (0.168)
Job loss %	1.701* (0.964)
Robbery %	-0.232 (0.433)
Death %	-0.570 (0.534)
Illness %	0.042 (0.204)
Money transfers %	-0.220* (0.118)
Under 16	0.159*** (0.038)
Over 60	0.025 (0.577)
Dairy village	0.198*** (0.058)
Felt village	0.138** (0.051)
Buddhist %	-0.322** (0.133)
Distance to province center	0.001 (0.000)
Constant	0.685** (0.250)
Observations	39
R-squared	0.715

Notes: Data Source: Soum survey. This table shows a regression to link various village-level characteristics to village risk. Robust standard errors in parentheses. ***, **, * denote significance at the 1%, 5% and 10% level.

Figure A1 Overview of the participating villages and provinces

Notes: This figure shows the geographical location of the 10 control soum centers (villages) as grey dots, the 15 individual-lending villages as dark blue dots, and the 15 group-lending villages as orange dots across the five Mongolian provinces (highlighted in light blue) where the experiment took place.

Figure A2 Timeline of the experiment

Notes: This figure shows the roll out of the experiment across the 15 group-lending villages (upper panel), 15 individual-lending villages (middle panel), and 10 control villages (lower panel). On each line, the red cross on the left-hand (right-hand) side indicates the day that the baseline (follow-up) interviews started in a village. The red dots indicate the median disbursement date for first-time loans in each village.

Figure A3 Assortative matching in the 15 group-lending villages

Quantile plot of the village sorting percentile for *Number of safe choices*

Quantile plot of the village sorting percentile for *Range of revenue*

Notes: These two graphs plot the default order values of the village sorting percentiles (y-axis) against their cumulative probabilities (x-axis). The sorting percentile is the percentage of all possible groupings of borrowers in the village that result in a lower value of the variable *Between* than the one observed. *Between* is a measure of group homogeneity and higher values indicate more homogenous groups within a village. In the first graph individual risk aversion is measured as the number of safe choices the respondent made in a lottery game. In the graph below individual risk is measured as the difference between the revenue expected in an extremely successful year and the revenue expected in an extremely unsuccessful year. The diagonal represents the random uniform distribution.